VGA
Institution

JOURNAL

OF THE

OCIETY OF TELEGRAPH-ENGINEERS AND ELECTRICIANS.

FOUNDED 1871. INCORPORATED 1883.

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

PUBLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE,

AND EDITED BY

PROF. W. E. AYRTON, F.R.S., CHAIRMAN.

VOL. XIV.—1885.

London:
E. AND F. N. SPON, 125, STRAND, W.C.

New York:
35, MURRAY STREET.

1885.

TABLE OF CONTENTS.

Vol. XIV.

VGA
Institution

JOURNAL

OF THE

SOCIETY OF

Telegraph-Engineers and Electricians.

Founded 1871. *Incorporated* 1883.

VOL. XIV. 1885. No. 55.

The One Hundred and Thirty-ninth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 22nd January, 1885—Professor W. GRYLLS ADAMS, F.R.S., President, in the Chair.

The minutes of the Annual General Meeting were read and confirmed.

The following Students were announced to have been transferred to the class of Associates:—

Charles Bright.
Henry Carfrae Seddon.
Edward Stallibrass.

The names of new candidates were announced and suspended, and

The SECRETARY [illegible] d that since the last meeting donations had been re[illegible] ibrary as follows:—John Aylmer, Esq., Member, [illegible] titution [illegible] sh Architects; [illegible] mber [illegible] the Society [illegible] n-P [illegible] nerican Bell [illegible] ofessor J. E. [illegible] he Cambridge

[illegible] f calling upon [illegible] at the Society's [illegible] "On the Relatio[illegible]

which should subsist between the Strength of an Electric Current and the Diameter of Conductor, to prevent Overheating." I am sorry to say that the instrument—a Kohlrausch galvanometer—chosen by Professor Forbes, as the Society's premium, has not arrived from Frankfort, and therefore I am not able to hand it to him, but I have great pleasure in handing him a letter representing the fact. I have also great pleasure in announcing that the Fahie premium has been awarded to Dr. Stone for his very valuable paper "On the Physiological Bearing of Electricity on Health." Members will remember how Dr. Stone came forward to help us in the Conference of last year at the Health Exhibition, and what a pleasant gathering we had there, when papers were read, one by Mr. Crompton in the morning, and then one by Dr. Stone in the afternoon. He of course occupied the more difficult place, because after three or four hours' discussion on the previous papers it might be somewhat difficult to keep up the interest. We know how well Dr. Stone succeeded, and the value of his paper has been recognised by the Council, who have decided to present to him the electrometer which you see on the table. The Paris Electrical Exhibition premium was awarded to Mr. H. C. Mance, to whom I have very great pleasure in handing this box of instruments, as a small token of the appreciation by the Society of the paper which he read before them "On a Method of Eliminating the Effects of Polarisation and Earth Currents from Fault Tests."

Now, gentlemen, having completed what may be regarded as perhaps the business of the past year, I have to vacate this chair; and in doing so I have to give you my thanks for the way in which you have supported me during my year of office. I think we have had very good meetings, and that these meetings have been well attended; and I am sure that members of the Society deserve my thanks for the way in which they have attended these meetings and communicated papers to the Society during the year. I cannot leave the chair without also giving my thanks to the members of the Council, who have supported me so thoroughly throughout the year. The members of the Council are always ready to do work in providing papers and in taking care that the Society has the right papers, and on them must greatly depend

the success of our Society; therefore I wish to present my thanks to the members of the Council for the ready support which they have given me. I now have very great pleasure in giving way, and calling upon Mr. Spagnoletti to take the chair and to give us his Address.

Mr. C. E. SPAGNOLETTI, M. Inst. C.E., then took the chair.

Mr. WILLIAM CROOKES: Mr. President,—May I be allowed to make the first speech during your occupancy of this chair. I rise to make a proposal that I am quite sure will be met with acclamation by all. I wish to propose that the hearty thanks of the Society be given to our late President, Professor Adams, for the way in which he has carried on the work of the Society during the time he has occupied that chair. We all know how well he has carried on the duties in this room; but speaking to you as one who has had the pleasure of working with him on the Council for some time, I can assure you that the work which Professor Adams does here, and which you see, is as nothing in comparison with the very valuable assistance which he gives the Society in the less formal work that goes on at the Council meetings and in Committee. I therefore beg to move that the best thanks of the Society are due to Professor W. Grylls Adams for the able manner in which he has discharged the duties of President during the past year.

Mr. E. GRAVES: I can add nothing usefully to the remarks of Mr. Crookes, and therefore beg to second the resolution.

Mr. C. E. SPAGNOLETTI (the new President): My first duty is a very pleasing one. It is, as you are aware, to ask you to accord to Professor Adams a hearty vote of thanks for the able way in which he has carried on the work of the Society during the time he has occupied this chair. It has been well proposed by Mr. Crookes, and concisely seconded by Mr. Graves, and I would ask you to accord it, as is customary on such occasions, by acclamation.

The resolution was unanimously carried.

Professor ADAMS: I am very much obliged to you for the kind way in which you have passed the vote of thanks, and I will only imitate the brevity of the proposer and seconder of the

proposition, by not taking up your time now, when we are anxiously looking for the words of wisdom which will presently fall from our President's lips. I thank you most heartily for the kindness which you have shown towards me.

INAUGURAL ADDRESS.

By C. E. Spagnoletti, M. Inst. C.E.

The next duty I have to perform is to thank you most sincerely for the great honour you have conferred on, and the confidence you have shown in me, by electing me as your President for the coming year. I also thank the Council very much indeed for their kind and unanimous nomination, and I am very grateful to you all, gentlemen, for your confirmation of that nomination.

Being on our Council the representative of that branch of our science which immediately applies, and is so important an adjunct to the working of our railways, the honour you have done me will, I feel sure, be felt by all my colleagues engaged in railway telegraphy, and they will appreciate this recognition. I am fully conscious of the attention and care that it is necessary to devote to the responsible duties of President of this growing and prospering Society, and I will do my best, with the kind support I know I shall receive, to properly carry them out. Each year this position will become more and more arduous to fulfil; its functions will increase, and become still more important, as our science grows and its branches develop and become more numerous, for there is no knowing what further great ends electricity may be the medium of effecting, or how soon some new line by its use may be struck out for us, and enable us to form important additions to our industries.

It is only a few years ago that we were small in numbers, struggling hard to make both ends meet; but by the successful efforts and perseverance of those who have had the conduct of our affairs, and their earnest desire to do their utmost for the welfare of the Society, we have become, and I trust may continue to grow, a still more important and influential body. We are now an incorporated Society, with a firm foundation to further build

on, with money invested, with our financial condition sound and good; and we are, too, in the very satisfactory position of being able to give prizes to our Members and Students, and so offer some reward to encourage and stimulate them to become useful and valuable members of the Society. There is plenty of good ground for them to work in by studying and taking up many subjects which require deep investigation, and then favouring us with the result of their labours, thus taking a very important part in contributing to our progress and knowledge, and increasing the value and capabilities of our Institution.

I should like here to take this opportunity of addressing a few words to our younger Members and Students; I would ask them to take a more prominent part, than they have hitherto done, in the proceedings of our evenings, and in the discussions on the papers read before the Society. In this Society, as in many others, it must be noticeable to all that for the majority of the papers read, and the discussions thereon, we are indebted to a comparatively few members, and the same voices are continually being heard. Now I do not for one moment wish in any way to subdue those voices,—I hope we may still have the pleasure of hearing them, and the good information they are so capable of giving us, for many years to come,—but we also want to hear others as well, and I am quite sure you will agree with me that it is hardly fair that the majority of work of the Society should be continually put upon so comparatively few of its members. It is a duty every member owes to the Society to do what he can to support and uphold it, and to assist those who have been elected (if I may so say) as guardians of the Society and hosts of the meetings. They have with care to see to and provide the evenings' entertainments, and to arrange that they shall be what they ought to be, and they would be only too glad to receive a little more external assistance; and I do most strongly urge upon you to consider this question. We want to widen the sphere of our Society, and make it as popular, entertaining, interesting, and instructive as we possibly can; and this can only be done with success by all its members giving their best assistance and help. We who sit here to-day will shortly require successors, and those

most useful to the Society will have to be elected. In taking part in these discussions there is no need for long elaborated speeches: an expression of opinion, a few remarks or a question on some point or points, may give a fresh impetus to the discussion, and this may bring out further enlightenment on the subject under consideration, which but for some such remark or question the Society might not otherwise have the benefit of. In writing a paper much time, labour, thought, and search is needed; but these efforts do not only benefit those who hear the paper read, or read it in our Journal, but is of far greater advantage to the author himself, as it necessitates his acquiring and seeking such knowledge and information of his subject which in all probability he would not otherwise so diligently and deeply search for. He will find as he proceeds that the desire to be very correct and more complete will grow stronger and stronger, and suggestions will arise which will lead him to greater results than he at first anticipated, and amply repay him for his trouble. In taking up a subject in this way, although mastery of it is frequently obtained, there may be still some obscure points which sometimes present themselves, and when it is under consideration and discussion, these are frequently brought out and made clear, and the author thus gets assistance in solving difficult and knotty points which but for such discussion might still remain in obscurity.

We are told "unity is strength," and in a young Society like ours there can be no question that the more the unity in imparting knowledge one to the other, the greater will be our strength and value as a Society. When a subject has been thoroughly digested and understood, the mind frequently sees where there is a missing link, or where its practical application could be directed to some new purpose; and the application of such an idea leads up to invention, which not alone, in many instances, is profitable to the inventor, but is of service, convenience, and benefit to the community at large.

Some classes of electrical experiments are very easily carried out, and in a very inexpensive manner. Look only at the simple appliances Faraday employed in his experiments in induction,

and see now to what those have led. A little ingenuity can surmount what at first may appear an insuperable difficulty; and we have other instances of the ease by which experiments of importance may be tried, not only most simple in themselves and easy to obtain, but which have led the inventor to marvellous results. We know from our own experience the exceedingly primitive and simple appliances that Professor Hughes manufactured by his own hands in his researches in the microphone, his induction-balance, and his experiments in magnetism.

We now have a magnificent Library, of which we ought to be very proud, and which possesses already a collection of from ten to twelve thousand books and papers, and these numbers are still rapidly increasing, thanks to our energetic and zealous Librarian; and from his very satisfactory report just issued it will be seen that we receive no less than 71 English and Foreign periodicals, that all specifications for electrical patents as soon as they are published are sent to the Library, and that during the last three months of the year just ended we had an addition of 123 volumes. It is satisfactory to find that the Library is appreciated, and that the number of members using it is increasing; and now that it is open until 8 p.m. four nights a week, greater opportunities are afforded to our members for its use.

I hope, in addition to this great source of knowledge, that we may be able soon to afford to our members and place at their disposal a Laboratory, where they may be able to try any experiments that they may desire, and likewise that this Laboratory may be of service to our Students in the course of their studies, and enable them to form a more practical and clear comprehension of any questions they may have under consideration than they can possibly obtain without such aid. I think this is a subject which should receive our early consideration, even if we have (until we can afford a free one) to make a small subscription towards its attainment.

I have often thought that the science of electricity is one which, from the delicacy (and I use this term advisedly) of its nature, might make to a greater extent provision for a growing national necessity: I mean the employment of female labour. It

would very well afford an opportunity for such labour in the laboratory and testing rooms, which work might, I think, be performed by ladies, if they were specially educated and trained for it; and the lighter and stereotyped work in the workshops of large establishments might, in such matters as coil-winding, dial-painting, etc., be well performed by less educated women, under proper supervision. The Post Office and Telephone Companies are considerable employers of female labour, and the result of their experience, so far as I am aware, has been very satisfactory in all respects. Women have also been employed successfully in the manufacture of incandescent lamps.

I will now turn to a branch of our science which I believe is one destined to become more popular and general than any other, viz., that of electric lighting. All who are interested in its advancement—and all surely will be so, as they become acquainted with its comforts and healthfulness—cannot but regret its present state of comparative stagnation. This I consider is partly due to its having been commercially "rushed" too soon, and before the necessary requirements were met for its being practically carried out, and the requisite provisions and powers were obtained from the Legislature for its successful conduct.

The first great exhibition of the electric light which brought it prominently before the public with something like success was the exhibition held in Paris in 1878, where it was first shown to any extent as a luminant for streets and public buildings. Gramme, Jablochkoff, Serrin, and Lontin came on this occasion well to the front, and had certainly made great advances. Very shortly after, American and English machines began to spring up and blaze forth their brilliant effect with similar success, and in such numbers that the public, having so many systems brought before it, and each doing its work apparently so well, were—I think naturally, and certainly prematurely—led to conclude that the problem of electric lighting was not after all so difficult a one, and could be successfully carried out. The advantages of such a light being so manifest, they were willing to accept it as an accomplished fact. Under such circumstances I do not think the subscribing *public* (many of whom, by the precipitate action of others, have

been great sufferers) are to be blamed for having felt confidence, investing their money, and taking an interest in what must have seemed to them such a pronounced improvement upon gas.

The second Electrical Exhibition in Paris, held in 1881, exceeded anything that had previously been attempted, and demonstrated with full effect the beauties of the electric light and its various applications, as well as the power of electricity for other purposes. This was followed by our Crystal Palace Electrical Exhibition, the Aquarium Exhibition, the exhibits at the Fisheries Exhibition, and, lastly, that of the Health Exhibition; and to these may be added other Continental and American Exhibitions, all of which were reported as great successes. These acted as further inducements to speculators. The result of this was that, money being so easily obtained, many companies were formed, very high prices were paid for patents, and consequently a too reckless system has greatly contributed to the present dilemma; and this, in my opinion, has been a stumbling-block to the progress of electric lighting. The companies that were formed in 1880 and 1881 spent large sums in patent expenses and installation, in order to show what they could do. This meant expenditure and no profit.

The question of distribution, and the system of registering the amount of current supplied or used, is not, even at present, in so satisfactory a condition as could be wished, and the perfecting of these systems can only be achieved by experiments resulting from careful watching and time. Great improvements in both these matters will be shortly forthcoming, and the experience gained by practical application of the systems, and large installations when they can be made, will soon afford the experience and knowledge required to surmount all the present difficulties that exist.

The Act giving the requisite powers to commence actual work was not passed until 1882, and by that time many of the companies had spent a very considerable part of their capital, and when this Act was passed two or three of its clauses were of such a nature as to effectually stop any further progress. These clauses require to be either repealed or so modified as to encourage and assist the system, instead of crushing it.

A company undertaking electric lighting should be enabled to

see its way to a fair and proper return on behalf of those who are disposed to advance capital to carry on such work. To this end committees have been formed, and petitions have been, or will shortly be, forwarded to the President of the Board of Trade, pointing out what clauses it is absolutely necessary to repeal or modify, both in the Act itself and in the Provisional Orders, to give the system a fair and proper start; and as very capable men constitute these committees, and are actuated alone by the same object, viz., to obtain the establishment of the light, the conclusions that have been arrived at by them may be taken to represent the views of those anxious to obtain the light and those desirous of supplying it, and it is to be hoped their representations to the Government will receive very careful consideration, and that the necessary concessions will be granted. In fairness I should state that the Act itself, with two or three exceptions, is not altogether an unsatisfactory one. The points that press most hardly on the companies are these:—1st, with regard to licenses. As it at present stands no license can be obtained unless the consent of the local authorities is first obtained, and forwarded with the application to the Board of Trade. Now, should it so happen that the local authorities were themselves the "undertakers," and their terms to supply a special district beyond the limits they desired to make were excessive and unreasonable, or if the local authorities were interested in the supply of gas, the chances of any one obtaining their consent for a license would, I take it, be extremely hypothetical. Now, in such cases as these, the withholding of the consent should not be allowed to veto the application to the Board of Trade for a license, as it would be giving a power to one interest to injure another, without a fair hearing of the case. There would also be an advantage, if in the granting of licenses, the holders, with the consent of the local authorities, could use the streets for a private supply, without being obliged to give any other supply. I believe that the clause referring to licenses has been somewhat erroneously construed; if this is so, a necessity is shown for amendment, for if it is ambiguous it should be so altered as to make it clearly and unmistakably convey its intended meaning.

2ndly, the question of meter measurement. At present there is a want of a sufficiently reliable meter; and as this is the case it becomes necessary, until time has given us what is required, and allowed of a practical test of such apparatus, that conditions should be made as will admit of the supply to be received not only through a meter, but be charged for per light; and as lamps vary, and the undertakers are better able than the general public to advise upon the class of lamp that should be used, in the interest of both parties, the former should be allowed to decide this point. The gas companies, I would remark, enjoy similar powers with regard to burners, and the necessity for such a regulation is obviously greater with electricity than with gas.

3rdly, clause 27, known as the "compulsory purchase" clause. This clause requires repealing or entirely remodelling, for as it at present stands it is the great obstacle and the barrier that bars the progress of electric lighting. The initial expenditure consequent upon the lighting of a district must of necessity be very considerable, and any company that undertakes such work must be prepared for this, and also for a continual expense until the system is made perfect, or nearly so. We all know in a new system that experience is only the pioneer for further development and invention, therefore it is but reasonable to make some allowances for such circumstances. When these large areas are lighted it will be comparatively new ground to work on, and although there is no doubt as to the ultimate results, it is of great importance that steps should be taken to lessen the cost of the supply. Now clause 27 provides that at the end of twenty-one years, on a six months' notice, the local authorities have the power of purchasing the undertaking (or such part of it as is within their jurisdiction) at the then valuation of the plant, buildings, works, materials, and lands of the undertakers, suitable to and used by them, within the local authorities' jurisdiction; and if part of the undertaking is only purchased, a consideration for the loss by severance is to be made, but without regard to compulsory sale, goodwill, or any profits that have been or may be made, or any other similar consideration; so that if a company, after years *of working, are just* beginning to make the business

pay and put it on a prosperous footing, the local authorities can, on a six months' notice at the end of twenty-one years, or on a six months' notice at the end of each succeeding seven years, elect to purchase it; but if, on the other hand, it is not a profitable concern, they need not do so until such time as it does pay, and is worth their taking over. Now, generally, such purchases are ruled by a capitalisation of profits, or outlay and goodwill is allowed, or it is purchased at the market price as a going concern; and it is, I venture to think, a great mistake to make an exception in this instance. If such conditions are to stand, then the period of time must be considerably extended, and the price to be paid should be a market value as a going concern. If any improved systems were to be brought out, it would probably be necessary, for successful competition, to purchase or rent them, and amounts that are paid as royalties or for patents would not represent any value after their expiration, and time therefore should be allowed, after such expenses, for a return of the outlay.

With regard to the Provisional Orders, there appear to be two points that call for revision: the first is, as to compulsory laying of mains; and the second, as to the power to require a supply which should be subject to a minimum of so many hours, or at a special rate per annum. In the first instance it could hardly be expected that a company should be called on to incur an expense when there is no prospect of getting any return for it, and as this is not required from the gas companies, it seems hard to impose it on the electric light companies; and, in the second case, it would be but fair to the "undertakers" for the consumer to state the amount of supply that is approximately to be taken by him, and if it is not likely to be a fair and adequate quantity, then I venture to think that in such cases it should be left to the "undertakers" to make special arrangements with such consumers. I believe the President of the Board of Trade is well aware how anxious people are to obtain the electric light in their houses, and as he will now receive advice on the subject from qualified persons, it is to be hoped that the obstacles that have hitherto retarded its progress may be removed, and that we may see it make a steady and satisfactory advance in the future.

Although the electric light has not yet progressed to any great extent in this country, it is cheering, from Mr. Preece's experiences, to hear that it is advancing in America satisfactorily, for this is paving the way for us.

I am very glad to find that the opinion I have formed as to the services each class of lamp should perform is being confirmed by the systems adopted in America. I think it is certainly wrong to attempt space or outdoor lighting by the incandescent lamps: they are essentially for internal lighting, as the arc is for outdoor lighting. When the Edison Company first opened their central station on the Holborn Viaduct, I went with Mr. Preece to see their arrangements; and, as Holborn was then being lighted by two or three different systems, we walked up it to see them. First we came on the ordinary gas lighting, which needs no remark; then we came on to Siemens' gas and air lamp, which lighted up the street well. We then came on Sugg's or Bray's triple burner: this also gave a very good light; and we lastly came to the two incandescent lamps of Edison, in the ordinary lamp-posts, which was just like going back to the ordinary gas lamps, if not worse: the effect was most disappointing and depressing, and we then came to the conclusion that this last named would certainly not gain favour for electric lighting, and that it was a great mistake. The plan adopted in America of lighting the squares by a tall mast in the centre is one that might be easily carried out. We have had an opportunity of judging of this mode of lighting by the masts at the Health Exhibition. The system of high masts has also been carried out at the Royal Albert Docks, where it appears to answer very well; and the London and South Western Railway Company have also adopted this system in their goods yard at Nine Elms Station, with good results. I had to enquire some time ago into this mast system, and the reports I received were in every way most satisfactory. I was told it facilitated the forming of trains, and thus ensured their punctual departure; and although I cannot say every light was equal to a policeman, as has been said, I was informed it had had the effect of preventing thefts.

Before leaving the subject of electric lighting, I would mention that there is another application of it that would be received with

very great pleasure by many millions of people—that is, the lighting of railway carriages. When you consider that the number of passengers in a year in this country is over 600,000,000, say one-sixth only travelled after daylight, the boon would be greatly appreciated. The railway companies would, I feel sure, be only too glad to afford a better light, but the difficulties in adopting the electric light are many, and I will point out those that prominently present themselves to my mind, so that whoever may be studying this subject will get an idea what to provide for, and the difficulties to be contended against. Of the systems at present under trial, one is with a dynamo and accumulators in the van of the train, worked from the axle; another, with the dynamo and an independent small engine fixed on the locomotive, taking its steam from the boiler. And primary batteries have also been tried. Now with any system where the source of generation is at one point, either on the engine, or in the van, you meet with these difficulties: if the supply is on the former, each time it is changed or detached for water, or the train is divided, the carriages are in darkness; and it is the practice at a terminus to have the coaches brought up to the platform sometimes twenty minutes or half an hour before the engine is attached to them. But if in the case of the latter, *i.e.*, the van, any dividing of the train to attach an extra vehicle would mean that while this was being done the train would be in darkness, or at any rate the portion of the divided train that was not carrying the power, and the lamps that were in the portion of the train from which the light was derived, would receive an excess of power which they would be better without. If accumulators are put in each carriage separately, for lighting it independently, and are charged either by dynamo on the engine or from the van, the difficulty may frequently arise that a carriage which has been standing for some weeks may be unexpectedly wanted, and when put into the train, it might happen that the accumulator it contained would not light the lamps until it had been recharged. What is wanted is some system that will meet all these requirements and details in railway *working*, and put the railway companies in possession of the

electric light with all the independent advantages of the existing lights, and with as many more as can be given. Train lighting is an expensive item in railway working, and there is a fair field, with a good margin for profit; but it should be remembered that the great competitor will be gas, which I understand is about half the cost of the system of lighting by oil more generally in vogue at present.

Up to the present time the use of primary batteries for electric lighting has not yet made any advance; many have been tried, but the continual attention they require will be a very serious obstacle to their use, even if their efficiency could well be maintained.

Accumulators, or secondary batteries, have not during the past twelve months made any great strides, but I believe this system will have a great future before it, as soon as a battery is in the market that can be relied on. Some that I have seen and tried require little or no attention, and after three years' use give no signs of depreciation in the materials. Secondary batteries appear to me to be a most essential element in electric lighting: they represent the gasometer of the gasworks, and a storage of electricity of any amount of current, with any reasonable amount of electro-motive force, can be maintained by them, and given out at a moment's notice, in any proportions required.

The commercial system of telegraphy in this country is still rapidly increasing, so much so that the Government have found it necessary to enlarge their office accommodation at their great central station; and not only has the business increased very considerably, but great progress of late years has been made in the efficiency of the system generally. Lines are more substantially built than they used to be; are better looked after; their electrical condition is more carefully maintained; and now that the telegraphs of the whole country are under one management (and certainly a very good management), opportunities are afforded that could not be given when the now one system was divided between three or four telegraph companies, in making duplicate or alternative routes, so that in cases of accidents the inconvenience of such events will be very slightly, if at all, felt by the public. The reductions in the rates for telegrams will no

doubt be the means of bringing a very considerable addition to the present large number of messages.

The pneumatic tube for a local system affords a ready means for the transmission of messages, and I believe it is contemplated to extend this system considerably, especially in London and Liverpool. Additional facilities are, I hear, to be given for transmission of messages to the Channel Islands, the Isle of Man, and other outlying islands. We see continually additional wires erected on the railways and along the roads; and I believe I am not wrong in saying that about 26,000 miles of wire are likely to be soon added to the great mileage already in use. This mileage is divided between about 900 miles of new poles, and 60 to 70 miles of underground pipes, and additional wire on existing poles. The "sounder" instruments seem to be gaining favour, and the old system of audible signals introduced by Sir C. Bright is rapidly developing in a new form, and will very probably be the general system in this country, as it is in America. From the foregoing it will be seen that our progress is great and rapid in commercial telegraphy; and the more general its use, the sooner it will become a greater necessity.

In our Colonies we do not appear to be making many extensions. I hear that the quadruplex system is being introduced in Australia, which indicates an increase of business and improvements there. I understand that the Wheatstone "automatic" is now being introduced in America, and that the instruments as used by our Post Office are most in favour; and it is gratifying to us all to feel that not only in America, but generally over the Continent, our systems are finding such favour, and that we take the lead in high-speed apparatus.

The telephone makes steady progress in this country and abroad, but is not used in any country as it is in America. Here this probably is due to some extent to our excess of boy labour and the facility with which a messenger or commissionaire can be obtained, our rapid means of transit, and moderate cab fares, together with a temperate climate, which during the summer is not so hot as to nearly melt the vitality out of us, and to make *exercise* really a subject of serious consideration.

Several experiments have lately been tried in this country with long-distance telephones, and in testing the "Bell" telephone with these it was found that it possessed advantages that were not so well understood before, and we now see the establishment of an exchange in Brighton working satisfactorily direct to London; this will doubtless lead to a wider sphere of action, and the further development of exchanges between large towns. The daily number of communications made in London, I learn, is about fifty thousand. The mileage of wire erected is about nine thousand miles, and about seven thousand instruments are at present fixed. There are twenty exchanges, but most of the subscribers are business men or commercial firms, and it is scarcely used here at all for domestic purposes and individual requirements, as it is in America. I fear the present price will prohibit its use for such purposes. The great convenience of this invention can only be felt by those who are in the habit of frequently using it, and there is little doubt that, as soon as the Company can see their way to a reduction in their charges, its use will be very largely increased.

The benefits of the great concessions made by the late Postmaster-General with regard to telephonic communication will no doubt do much for its extension. The prolonged time for the licenses, with no given distances, affords freer action for its introduction; and the release from royalties upon private wires, with the opportunities for connections with post offices, must undoubtedly do much for its future progress. Whether it will be profitable to adopt the system now being tried in France, Belgium, and America, of charging the general public so much "per chat" for so many minutes, can only be proved by trial.

The litigation which has been carried on so long as to the "Drawbaugh" patent in America is at length settled in favour of the "Bell" Telephone Company, and is only another case showing the enormous amount of money that is expended in settling rights which inventors have imagined they had already secured by paying the requisite fees for protection; and I hear, now that this case is settled, another action has to be fought.

The "Rysselberghe" system for getting over induction, and rendering ord*inary telegraph wires fit* for telephonic purposes,

without any interference with communication passing on the former, although of very great advantage, has only been applied as yet to a limited extent.

Improvements in the system of transmission of power do not appear to have been very great, and but little has been added to our knowledge during the last twelve or eighteen months in this direction. The experiments of Mons. Marcel Deprez between Creil and Paris have not as yet led to any practical result, in spite of his praiseworthy persistence in them; nevertheless, of late we have seen many attempts to utilise the power of electricity for locomotion. Electrical rail and tramways have been tried both at home and abroad, with every sign of progressive success, and the practicability of the system has been clearly shown by the lines that have been so worked in Paris, Berlin, Frankfort, Switzerland, Brussels, Ireland, and America; and I hear many more such experiments are to be tried, and this system will, I expect, in a few years become a general one, and of great convenience.

Professor Fleeming Jenkin's system of telpherage, which was so lately shown on an experimental scale, is now beginning to grow into use, and the Telpherage Company appears to be establishing this system. They have improved their machinery by having now a break and governor, which, by tests, have proved satisfactory; and they also are in a position to accept contracts for the erection of telpher lines for the transit of goods and merchandise. One contract they have already obtained with the Sussex Portland Cement Company, by which they have contracted to deliver two hundred tons of clay per week for twelve months. The length of the line is about one mile. Another negotiation is well forward, I understand, for the conveyance of slag from blast furnaces to the tipping bank; and if this succeeds, as there is every reason to believe it will, large demands for telpher lines are anticipated. Many enquiries are being made from the Colonies, but, as the system is so young, the Company are wisely desirous of having home experience before they send any of their apparatus abroad. One circumstance alone *has just occurred* that will give great help to this system, and

that is, the Telpherage Company has been accepted as one of the Electric Consolidation in the United States. This certainly looks well, and I mention these facts to show that another new line may be opening for electricians that may need the services of many of our young members; but the system of transference of power for longer lines and heavier work, such as railways and tramways, will have a strong opponent in the system of working by accumulators:—the former presents many grave and serious difficulties, and the loss by distance, and the want of a good system of a lasting insulation, is exceedingly detrimental for any large development of it; but in the latter system—that of accumulators—it does appear a more simple process to charge and carry the store of power in a train with you. Great improvements in the manufacture of accumulators are being made, which will weigh in their favour for such work. Short lines of what are called "light railways," between stations and towns where two or three or more miles may separate them, could easily be made, and worked electrically by either of these systems, where steam is objectionable, and considerable convenience by them may be afforded to those whose residence is in the country, and some distance from the railway station.

Railway telegraphy continues to steadily increase and progress. All lines of any importance have their complete systems of block telegraph; and now that the facilities which the telegraph affords to the working of railways are better understood by the railway authorities, improvements are received with greater interest, and this affords the telegraph engineers better opportunities and more encouragement to continue the introduction of appliances which have been brought forward with such good effects. No one who has not had experience on a line of railway can form any idea of the care and attention that is required on all points connected with the safe working of the traffic. The results of what has already been done by electricity, in conjunction with mechanical appliances, will be shown by the figures I shall presently give you.

The following applications of electricity will afford to some extent *an idea of its various capacities.* In addition to the

general through and local circuits for the ordinary business, there is the "Block system." There are also indicators attached to the distant signals when out of the sight of the signalman who works them, so that he may see by these indicators the position of the signal-arm at the signal-post—whether it is properly up at the right angle for "danger," whether it is down in the right position for the "all right" signal, or whether any contraction, expansion, or breakage of the wire has failed to place the arm of the signal in the exact position required. These instruments also show instantaneously any defects that may arise in their own working. As this apparatus only applies to the day signals, and as it is just as necessary that the signalman should know when the light for night signals is properly burning, there are lamp indicators which show whether the lamp is "in" or "out." At level crossings where there is much road traffic, electrical indicators are fixed which give notice to the gate-keeper when trains are leaving the station on either side of him, as a safeguard to both railway and road traffic. There are also appliances which are attached to drawbridges over rivers or docks, and by which the bridges control the telegraph in the working of these systems. It may be interesting to learn how this is accomplished. An absolute rule is made to ask for a signal for a train to be allowed to proceed over these bridges, and no train is allowed to start until the proper audible and visual signals in reply have been received; but should the bridge be out of position, a reply cannot be returned, as it is only when the bridge is quite ready for the passage of the train over it that the telegraphic communication is completed. There are likewise appliances which are fixed to water tanks, communicating with the man at the pumping station when he is any distance from it, so that as soon as the tank is full the water actuates an apparatus on the principle of a ball-cock, which rings the bell at the pumping station as an indication to stop the engine, thereby preventing waste. These can be made not only to indicate when the cistern is full, but also when it is empty. Similar electrical tell-tales are also used as indicators to gasometers, showing when they require refilling or are full. Electrical appliances to convey to signalmen situated at the ends

of large station yards are used to inform them which trains are coming, and from which platform they are about to start, so that the right points may be turned, and the proper signals lowered for the train to proceed; and as the mechanical interlocking system is now used, very great security from accidents is afforded.

The telephone is now becoming greatly used by railway companies, and on some lines it is employed between the signal boxes at many places. The next electrical appliance that is likely to be generally adopted is the system of electrically locking the starting signals from one station to another, and by making the train itself perform the duties necessary for completing the signals and restoring the electrical circuit for a second signal to be sent. By this system a signalman cannot lower his starting signal for a train to proceed to the next station until the signalman at the station in advance has (in reply to an arranged bell signal) unlocked the signal lever. An indicator shows when this is done. The lock having been taken off enables the signalman to lower the signal, and on putting it again to "danger," after the train's departure, to protect it, he cannot lower the signal for another train to follow until it is unlocked by the man at the station in advance, who is unable to do this until the train has passed a point selected by the proper authorities. When the train passes this spot, the apparatus in the signalman's box by which he unlocks the signal at the station in the rear for another train to follow is reset by the deflection of the rail on the passing of the train, and he is then enabled to use it again as required. By this means it will be seen that the men at each end cannot make a mistake by letting a second train follow while the first is in the section and is itself the medium which controls the action of the men at both ends until it is clear of the section. In applying this system to single lines it has to be arranged that not only shall one train not follow another until the section is clear, but that no train can be started in the opposite direction, and this is very satisfactorily and easily provided for. The system of electrical locking can also be applied at junctions, at sidings between stations, at level crossings for locking gates, and, in fact, in many other ways. At junctions it can be so arranged that, no matter how many lines converge, only one

train at a time can be allowed to approach the junction, so that when a train has been "accepted" none of the signals on the other lines can be lowered until the train has passed the given point.

There is also a very ingenious electrical machine for working single lines of railway, which consists of a box containing tablets at each end of the section, and it is necessary for the driver of the train to have one of these tablets before he starts: it is given by the station agent to the guard, and by him to the driver, so that both guard and driver know they have the authority to start when all is ready. But before the agent can get one of these tablets out of the box he has to communicate to the station in advance, and the responsible person there, by sending a current, releases one of the tablets, which is given to the driver; and before another tablet can be released at either station for a train to proceed in either direction, the tablet which the train occupying the section possesses, must be placed in the box on its arrival at the end of the section so worked.

By these various appliances it will be seen that electricity plays a very important part in the working of our railways. Several of the railway electrical engineers have introduced useful appliances of various kinds, and they will no doubt, by their knowledge of both railway working and requirements, together with their electrical experience, greatly contribute to develop such systems which will further tend to lessen the chances of the very few accidents in railway travelling.

I find by comparative returns of the Board of Trade between the years of 1873 and 1883 the following results, and for which I think railway telegraphists may claim their fair share of the credit due to such improvements in the safe working of railways:—

		1873.	1883.
Total length of Line open for traffic, United Kingdom—Miles		16,082	18,681
Number of Passengers		455,320,188	683,718,137
Tons of Merchandise		190,953,457	266,362,968
Train Mileage		197,354,749	268,897,236
Number of Passengers killed	From causes beyond their own control.	40	14
" " injured		1,522	58½
Number of Companies' Servants killed ...		52	12
" " " injured ...		294	56

These figures will convey to you some idea of the great responsibilities devolving on railway directors and officers; and when we compare the numbers of passengers carried (nearly six hundred and eighty-four millions in a year) with the number killed (fourteen) and injured (five hundred and eighty-four) from causes arising beyond their own control, we cannot but feel great satisfaction with the excellent management which is productive of such results.

I cannot conclude without apologising for the length of my address, and for the various topics it treats of. My object, however, has been to show how numerous are the subjects of interest and importance to our profession in the present day, and what a vast field they offer to our members, both in remunerative employment as well as in scientific research. As in all matters of life, there is no successful issue without hard study and perseverance; and I would again remind the younger members of my audience of the great necessity there is to exercise these qualities to the utmost, and thus ensure, not only to our Society, but to the world at large, those unspeakable benefits that arise from the practical development of science.

Sir CHARLES BRIGHT: I wish to ask you to give a very hearty vote of thanks to our President for the address which he has delivered to us. The actual wording of the vote of thanks I will give to you in a few moments. It happens to be the good fortune of this Society to combine within it, not only members who give themselves to the cultivation of one branch of the science, either as philosophers or as practical men, but to have a mixture of every representative of the applications of electricity among us. We have had the good fortune to have a man whom I should call a philosopher leaving the chair just now. I am speaking of our friend Professor Adams. And I do not call him only a philosopher, because a man can scarcely be a philosopher without combining in a great degree a knowledge of the practical applications of the science to which he is devoting himself. Well, before that we had the advantage of enjoying the presidency of my old friend Mr. Willoughby Smith, *with whom I* have had the pleasure of

co-operating with in various works, and whose friendship I have enjoyed for many years. Now this year, again, we have our friend Mr. Spagnoletti, who has given us an address that has some things in it which I consider very remarkable, and beyond the usual scope of presidential addresses, because he gives us his practical experiences on certain points which I consider to be of the greatest value, not only for us, but to be put before the public. I am referring for the moment to the things of which he has been speaking in the latter part of his address, viz., the provisions for the safeguard of individuals, not to say of property, in the applications of electricity to railway purposes. And I myself, being engaged very many years ago in the application of telegraphy in this country, can appreciate perhaps better than some of us here in this room the development of the application by railway telegraph engineers of electric signalling systems on railways, through which I may say we have between every station safeguards (barring any accident which you cannot help in the best appliances which you may devise) for the preservation of life. We have Mr. Spagnoletti, our President; we have Mr. Preece; we had also Mr. Walker. I speak of these names as being the greatest names I know of in the history of telegraphs as applied to railway purposes, who have done their best to preserve ourselves and our families while travelling on the lines. But I fear I am making my remarks a little too long. What I intended to say was to ask you to give a very hearty vote of thanks to Mr. Spagnoletti for his address, and I heartily congratulate you on having him as the President. I therefore propose "That the best thanks of the Society are due to the President for the very interesting address just delivered by him, and that he be requested to permit it to be printed and published in the Journal of the Society."

Professor HUGHES: Sir Charles Bright has fully expressed our appreciation of the most admirable address that we have heard to-night—an address showing the vast field now occupied by the practical application of electricity, and giving most valuable information as to the conditions necessary to success. It therefore gives me the greatest pleasure to second the vote of thanks which has been proposed by Sir Charles Bright.

Professor Adams: Our President has brought before us some very interesting topics in his address. Among others was one very interesting question—the lighting of trains by electricity; for our President assured us they would be lighted, whereas now there may often be some doubt whether there is any lighting at all. I have no doubt that lighting by electricity will be more costly than the system sometimes adopted, where very little light is given. There was also one other point in the address which touched us all very closely, and that was the statement by Mr. Spagnoletti that the electric lighting will ensure the punctual departure of trains. Now, those who have been accustomed to travel by the Inner Circle Railway since it was opened would very gladly welcome the electric light; for now making the way more easy, by sending the trains all round the circle, seems to have had the effect of delaying them ten minutes in a twenty minutes' journey. I have very great pleasure in putting the proposition, and in asking that the address be allowed to be printed.

The resolution was carried unanimously.

The President: I have to thank you very much for the attention you have been good enough to give to my address, and for the manner in which you have received the kind remarks made by Sir Charles Bright and Professor Hughes, in proposing and seconding a vote of thanks to me for my humble efforts to-night, and for which I thank them.

I am sorry to say that I am suffering from a very bad sore throat, and am not able to do it quite the justice I should like to have done.

I am requested to say that one of our members, Mr. Alfred R. Sennett, is very anxious to obtain information with regard to deaths or accidents that have resulted from electricity; and as it is difficult for him to find out to whom he can apply, I name it here, so that any gentleman present who may have (or may know anybody possessing) information on the subject, will oblige by leaving his name and address with the Secretary, so that a communication may be sent to him. I believe that Mr. Sennett is preparing a paper *in conjunction* with Dr. Stone, and we may

hope to have the benefit of any information he has, and that can be given to him, and I am sure that such information will be well worth our attention.

A ballot then took place, at which the following were elected:

Foreign Member:

Professor G. F. Barker.

Member:

J. F. Albright.

Associates:

Major T. H. Anstey, R.E.
Lieut. J. H. Cowan, R.E.
W. H. Cottrell.
Stanley Currie, B.A.
Arthur B. Gill.
Hartley Gisborne.
Frederick D'A. Goold.
Lieut. H. de H. Haig, R.E.
B. McMullen.
John D. Miller.
F. F. Yeatman.
Gustavus Töbel.
James Shepherd.

Students:

F. Gordon Jackson.
William F. Madge.
C. P. Sparks.
W. H. Wickham.
F. S. Worsley.

The meeting then adjourned until Thursday, 12th February, 1885.

The One Hundred and Fortieth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 12th February, 1885—C. E. SPAGNOLETTI, Esq., M. Inst. C.E., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The PRESIDENT: It is with feelings of very great regret that I have to announce (as one of my first duties) the death of a gentleman who was very intimately connected with telegraphy in its early days. I refer to Dr. Davey, who died last Sunday in Malmesbury, in the colony of Victoria. I dare say most of you are aware that he made many experiments and discoveries in early telegraphy, although his meritorious efforts were not well known to us for many years afterwards, and for which information we are indebted to Mr. Fahie. It was only late in the season of last year that the Society, in order to recognise those efforts, conferred on him its honorary membership, and I am sure it will be a matter of regret to us all that he has not lived longer to enjoy it.

The following Students were announced as having been transferred to the class of Associates:—

Paul Dimier and Alfred E. Ruddock.

The names of new candidates were announced and suspended.

The SECRETARY announced that donations for the Library had been received as follows, and thanks were awarded by the meeting:—J. Angelo Fahie, Member; R. Von Fischer-Treuenfeld, Member; J. A. Berly, Associate.

The PRESIDENT: I have much pleasure, indeed, in stating that the Council, considering that this Society is the representative of electricity and electrical engineering in all its branches, and having regard to the very slow progress that electric lighting is making in this country, have thought it not only desirable, but necessary, that it should enquire into the cause that is impeding the progress of this branch of our science. For this purpose a Committee has been elected. That Committee has met, and made its first report to the Council, which was considered and

approved to-night. The Council propose to send this to the Board of Trade, and, in order that you may know what is passing, the Secretary will read to you the resolution that we propose forwarding from the Society, with the hope that we may be able to help this struggling branch of our science against some of the difficulties with which it is now contending.

The SECRETARY then read the resolution, which was as follows:—

"Resolved unanimously, that the Society of Telegraph-Engineers and Electricians have seen with regret, that since the passing of the Electric Lighting Act of 1882 public lighting by electricity has not made that advance in this country which was confidently anticipated, and hence there has not been that stimulus to progress in many of the branches of electrical science which might have been expected, judging from the history of submarine telegraphs.

"That the Society, having had the matter under careful consideration, have come to the conclusion that the restrictions in the said Electric Lighting Act have in a great measure contributed to check the advance of public lighting by electricity, and they venture to request the President of the Board of Trade to consider whether the said restrictions may not be modified or removed."

The PRESIDENT then called upon Capt. H. R. Sankey, R.E., to read his paper.

ON SOME EXPERIMENTS IN ELECTROTYPING WITH A DYNAMO-ELECTRIC MACHINE, CARRIED OUT AT THE ORDNANCE SURVEY OFFICE, SOUTHAMPTON.

By Captain H. R. SANKEY, R.E.

About a year ago I was ordered by Colonel Stotherd, R.E., Director-General of the Ordnance Survey, to carry out some experiments in electrotyping with a dynamo-electric machine. These experiments were brought to a close in December last; and, Colonel Stotherd thinking that there might be some points of interest to this Society, I have been requested to prepare a short paper on the subject.

The electrotype process has been in use at the Ordnance Survey Office for the last 37 years, to produce duplicates of the engraved copper plates. About 700 copies can be printed off such electrotypes, but as a rule only 15 or 20 copies are pulled off at one time, just enough in fact to keep pace with the demand; and from time to time corrections and additions, such, for instance, as new railways, are made on the duplicate. It follows that the electrotype copper must be of such a quality as to admit of being engraved upon with ease—that is, it must be homogeneous in texture, without any trace of grit, and as hard as possible. A soft copper is not so pleasant to engrave upon as a hard copper, and, moreover, the plates would soon deteriorate under the action of the press. The process of backing electrotypes with soft metal has not been employed on the Ordnance Survey, and the practice has been to deposit copper until the matrices are about $\frac{1}{16}$ inch thick and the duplicates $\frac{1}{8}$ inch thick; and in the case of a duplicate it is necessary that the deposit be of uniform thickness throughout, to enable the plate to be passed through the press at a uniform pressure. Clearly, also, the copper must be able to withstand this pressure. The plates to be deposited upon are large, probably larger than usual in electrotype operations, but, on the other hand, the surface to be deposited upon does not vary considerably, as will be seen by the following table:—

Size of Plates.		Area to be deposited upon.
1′ 3″ × 1′ 10″ 1′ 9″ × 2′ 3″	together	6·2 sq. feet.
2′ 2″ × 2′ 8″		5·8 ,,
2′ 2″ × 3′ 3″		7·0 ,,
2′ 4″ × 3′ 0″		7·0 ,,
2′ 4″ × 3′ 4″		7·8 ,,
3′ 0″ × 3′ 4″		10·0 ,, (rare.)

From the preceding, it appears that electrotyping as practised at the Ordnance Survey Office is subject to the following conditions:—

1. The deposited *copper must* be homogeneous, without grit,

as hard as possible, and capable of withstanding the pressure in the press.

2. Thick deposits are required, and in the case of duplicates the deposits must be of uniform thickness.

3. The surface to be deposited upon is large, but does not greatly vary.

The old method, which is still in operation, fulfils these three conditions, and a short description of it may be of interest. This method consists of employing one Smee's cell to each depositing bath. These cells are of large dimensions, namely, 2′ 4″ × 3′ 3″ × 6′ 0″ deep, the plates are 2′ 0″ × 2′ 5″, so that there is about 4′ 6″ of solution below the bottom of the plates; the object being that the zinc sulphate may sink below the plates as it forms. These cells require frequent "livening up," and every three months have to be entirely remade. There are 9 such cells in use, 3 in one battery-room, and 6 in another, and the work connected with them is very onerous. Each bath contains either 1 large receiving-plate or 2 small ones, mounted on a board, and of course 1 dissolving-plate. The bath and its cell are placed as close as possible, separated by a thin brick wall, and leads of 7 No. 12 B.W.G. copper wire are used. To insure a uniform thickness in the deposit, the plates are placed horizontally, and the solution is kept in motion by rocking the baths. It has been found necessary to place the dissolving-plate above the receiving-plate, an arrangement which has the disadvantage of allowing any impurities there may be in the dissolving-plate to be precipitated by gravitation on the receiving-plate. The reverse arrangement has been tried, but air-bubbles form on the receiving-plate and the solution crystallises on the dissolving-plate. Before being placed in the bath, the receiving-plate is prepared as follows:—The surface is cleaned with rotten-stone, then with turpentine to remove the oil, and lastly with a solution of common soda and slacked lime, after which the separator, consisting of a thin wash of cynide of silver and of iodine dissolved in alcohol, is applied.

The depositing solution employed is the ordinary acidulated *sulphate solution*.

The results obtained by this process are excellent, and the only real difficulty is in procuring copper of sufficient purity for the dissolving-plates. It is, however, an expensive process, and, as already remarked, the work in connection with the cells is very onerous. Moreover, the cells themselves were nearly worn out, the lead lining being eaten through in many places, and the slate sides attacked by the acid. It was therefore decided to experiment with a dynamo machine, with a view of replacing the cells by it. Prior to my being placed in charge of the electrotype department, arrangements had been made, in August, 1882, with Messrs. Elmore to carry out some experiments with one of their dynamos. A plate was sent up to London to be deposited upon, but the result was unsatisfactory, due, it was said, to the faulty preparation of the receiving-plate. Messrs. Elmore then sent one of their dynamos to the Ordnance Office, Southampton, and with it deposited, in the first instance, on one plate: the resulting copper was granular and without cohesion. A further experiment was then tried by depositing simultaneously on 10 plates, but again the result was unsuccessful, in that the copper was not homogeneous. The result was, however, so far an improvement on that of the previous experiments as to warrant the purchase of the dynamo with a view of carrying out further experiments ourselves. Messrs. Elmore's experiments came to a close in September, 1883, but it was not until January, 1884, that any further steps could be taken, when I was ordered to continue the experiments, which will now be described.

It is known that the quality and texture of deposited copper depends on the rate of deposition, or, in other words, on the density of the current, and that, if the deposition be very slow, the resulting copper will be hard and brittle; but on the other hand, if the deposition be rapid, the copper becomes granular and wanting in cohesion. It is also clear that if thick homogeneous deposits are required, the rate should not vary to any appreciable extent during the deposition. Therefore, to fulfil *our* requirements—

1. The density of the current must lie between certain limits.
2. The current *must be kept* constant during the deposition.

As regards the first condition, no published information could be obtained at the time (lately some few results by Mr. Sprague have been found). It was therefore decided to measure the current flowing in our existing baths: this was effected by means of one of Cardew's ammeters, and it was found that in different baths the current varied from 23·4 to 26·4 ampères on plates of 7·7 square feet surface. As a check, a small plate $\frac{1}{18}$ square foot was prepared, and copper deposited upon it by means of a current of $\frac{1}{5}$ ampère, being at the same rate as 27·7 ampères on the large plates. The resulting copper was, to all appearances, the same as that deposited in the large baths. Moreover, about $1\frac{1}{2}$ lb. of copper is deposited on an average in 24 hours in each bath, which corresponds to a current of 24 ampères.

It was therefore decided to so regulate matters that the dynamo would send a current of 30 ampères through each bath.

To fulfil the second condition of keeping the current constant, a very steady high-speed engine, regulated by a very sensitive electrical governor, is clearly the desideratum. After an inspection of most of the principal high-speed engines now manufactured, and careful consideration, Willans' high-speed six-cylinder compound expansive engine was selected as being the best suited to our requirements. Mr. Willans had also just patented an electrical governor, and the engine was accordingly provided with one of these. It is not proposed to describe this governor, as Mr. Willans has submitted a paper to be read before the Institution of Civil Engineers. Both the engine and the governor have been found to work admirably, and have fulfilled all expectations. The governor is exceedingly sensitive, and yet does not flutter; it is, in fact, dead-beat.

The next point for consideration, was whether the depositing baths should be arranged in

Parallel,
Series, or
Combined parallel and series.

The more usual arrangement of placing the baths in parallel did not appear to be suitable, owing to the great difficulty, if not *practical* impossibility, of regulating the current in each bath

with that accuracy required for our work, because the baths have not quite the same resistance, and more especially because the resistance of any particular bath is not constant; so that, even if the main current were regulated, the current in any particular bath would vary. Moreover, this arrangement was adopted by Messrs. Elmore in their experiment with 10 plates, and, as already mentioned, the result was unsuccessful.

The combined arrangement is open to the same objection, though perhaps not to the same extent. As far, therefore, as the regulation of the current is concerned, it is preferable to place the baths in series; and it may be added that this arrangement was recommended by Mr. Willans when the question of winding the solenoid of the governor was discussed. It is also to be noticed that incidentally the advantages of being able to place the baths at a greater distance from the dynamo, of using smaller leads, and of having less difficulty in making connections, are gained by placing the baths in series.

Accordingly it was decided to adopt this arrangement if the dynamo would admit. It was therefore necessary to ascertain—

1. How many baths would be required.

2. What electro-motive force is necessary to produce a current of 30 ampères through this number of baths placed in series.

3. Whether the dynamo could be altered so as to produce this electro-motive force and 30 ampères.

To calculate the number of baths required, it is considered that the total output should be 120 lbs. of deposited copper per week, and as the dynamo will work about 44 hours per week, it follows that*

$$x \times \frac{30 \times 44}{384} = 120,$$

or

$$x = 35 \text{ baths.}$$

To ascertain what electro-motive force is needed, the resistance of a depositing bath and the counter electro-motive force of a bath must be known.

---

* $\frac{\text{Current}}{372}$ = weight of copper in lbs. deposited in one hour is a convenient formula derived from electro-chemical equivalent of copper. (0·003281 by Lord Rayleigh.)

The resistance of our baths is too small to be measured with accuracy with the ordinary Post Office pattern Wheatstone bridge. A wire bridge with sliding-contact was therefore made. The sliding-contact moves on a German silver wire, No. 9 B.W.G., and the known resistance consists of three German silver wires connected in parallel, and having a resistance of exactly 0·01 ohm. A reflecting galvanometer was used, and it was found possible to measure 0·001 ohm to within $\frac{1}{100000}$ part of an ohm. All the resistances in circuit being small, it was necessary to use a low-resistance cell, and one of the depositing cells was therefore employed. A commutator was also placed in the circuit, so that the current through the bridge could be reversed at pleasure, in order to annul the disturbing effect of the polarisation of the bath by the measuring current. The bath was connected to the bridge by very short thick leads, and as the bath and measuring cell were at some distance from the test-room, and the bridge had of necessity to be as near the bath as possible, the sliding-contact had to be moved by an attendant, who also reversed the current by means of the commutator. Communication was established by means of signals on electric bells. Considerable difficulty was found in taking this measure of resistance, and the results were not as satisfactory as might have been wished; they sufficed, however, for all practical purposes. The average of several determinations is 0·0087 ohm with plates 7·7 square feet area, and $\frac{3}{4}$ inch apart.

The counter electro-motive force was measured by noting the deflection produced by a bath on a reflecting galvanometer immediately after the depositing cell had been disconnected, and then noting the resistance through which a Daniell cell produced the same deflection, the $\frac{1}{999}$ shunt being used. It was thus found that the counter electro-motive force varied from 0·005 to 0·015 volt.

It therefore appears that each bath requires an electro-motive force of

$$30 \times 0{\cdot}0087 + 0{\cdot}016 = 0{\cdot}276 \text{ volt};$$

so that the total electro-motive force required is

$$35 \times 0{\cdot}276 = 9{\cdot}7 \text{ volts},$$

or, say, 10 volts.

Before mentioning the experiments carried out to ascertain whether the dynamo could be altered so as to give 10 volts and 30 ampères, it will be as well to describe the dynamo and the apparatus used for measuring the current and the electro-motive force of the dynamo.

The dynamo has somewhat the appearance of an alternate-current machine: its armature consists of a disc of cast-iron 1′ 6″ diameter, on each side of which are fixed 10 coils wound with No. 7 B.W.G. copper wire, and having each a resistance of 0·00297 ohm. As originally arranged, one set of the commutator segments (alternate segments) were connected to two rings, one on each side of the armature disc, but insulated from it; to these rings one end of each armature coil was connected, the other end being connected to the armature disc, which in its turn is connected to the remaining segments of the commutator. It will thus be seen that, as originally arranged, the armature coils were in parallel, so that the resistance of the armature was very low; in fact it appears by calculation to be 0·000149 ohm; and further, that the electro-motive force must be very low, being that induced in one coil. The field-magnet coils are similar to the armature coils, but larger, wound with the same sized wire, and each coil has a resistance of 0·0347 ohm. These coils are fixed to the cast-iron sides of the dynamo, and the armature revolves between them. The poles on each side are alternately north and south, and a north pole of one side faces a south pole of the other side. As originally arranged, the field-coils were connected in combined parallel and series—that is, each two consecutive coils formed a pair connected in series, and these pairs, ten in number, were placed in parallel: the resistance of the field was therefore $\frac{2}{10}$ 0·0347 = 0·0069 ohm.

The armature, fields, and circuit were placed in series.

It will be observed, since the ends of the various coils can easily be got at, that by joining in series, or in combined series and parallel, the electro-motive force of the dynamo can be increased up to a certain amount. It is also possible to arrange the fields in shunt with the circuit.

The commutator is not merely a collector, as in most dynamos,

but actually commutes the current; consequently the brushes can be arranged in different positions on the commutator so long as they bear on alternate segments. Should they, however, be displaced, so that even for an instant they bear on the same set of segments, they will begin to flare, and the current in the circuit will be considerably reduced. The same result will occur if the points of one or both brushes are not placed exactly parallel to the segments. A very small deviation from the true position of the brushes is quite sufficient to prevent any current being developed. In consequence, the adjustment of the brushes has been and is still the greatest difficulty. At first they had to be adjusted continually, and not the least dependence could be placed upon them. By altering the brush-holders and making them more substantial, better results have been obtained, but still very far from perfection.

The measuring apparatus consisted of—

1. A current-meter (Cardew's).
2. A potentiometer.
3. A Wheatstone bridge.

The test-room was placed at some distance (20 yards) from the dynamo, and the current was conducted by means of leads made of 7 No. 12 B.W.G. copper wires, having altogether a resistance of 0·0069 ohm.

A coiled iron wire resistance was introduced into the circuit, by means of which 0·92 ohm could be placed in circuit, proceeding by differences of 0·0058 ohm. This resistance represented the baths, and the circuit also passed through Cardew's ammeter.

Poggendorff's method of measuring difference of potential was employed, and the electro-motive force of the dynamo was balanced against that of 2 Daniell's cells. This potentiometer was connected with the terminals of the dynamo by thin leads of No. 20 B.W.G. copper wire.

The Wheatstone bridge was of the ordinary Post Office pattern, and of course the circuit was only placed in connection with the bridge when the dynamo had stopped running.

The test cells employed, both with Cardew's ammeter and the *potentiometer,* were Daniell cells, having two porous pots, as

recommended by Captain de Wolski, R.E. Both porous pots contain zinc sulphate, and the zinc element is placed in the inner pot. As a further improvement, a thin zinc cylinder, disconnected from the zinc element, was placed between the two porous pots, to precipitate any copper that might find its way through the outer porous pot. This cylinder has occasionally to be changed for a clean one. The electro-motive force of these cells was at first measured by the condenser method against a Latimer Clark's standard, but lately against a standard Daniell, made up with plaster of Paris, and it may be mentioned that no practical alteration in the electro-motive force of these cells has been noticed. Their liquid resistance was measured by Mance's method.

A code of signals was established, and worked on single stroke electrical bells, to enable the dynamo to be started, stopped, or the speed altered by signal from the test-room.

The first experiments were made without altering the original arrangement of the dynamo, but it was found, as for that matter was expected, that the resistance of the leads and connections (namely, 0·057 ohm) was too great, and only a very small current was developed, which was, however, not measured.

The armature and field-coils were then placed in series, when it was found that a current of about 100 ampères was developed through an external resistance of about 1 ohm at 600 revolutions.

The following arrangements were then successively tried:—

a. Armature, fields, and circuit in series.

(1) Armature—two sets of ten coils in series, the two sets being in parallel. Fields in series.

(2) Armature as in (1). Fields—two sets of ten coils in series, the two sets being in parallel.

b. Fields placed as a shunt to circuit.

(1) Armature in series.

(2) Armature—two sets of ten coils in series, the two sets being in parallel.

The last arrangement was found to be the most suitable for our purpose, as, in the first place, the desired electro-motive force and current can be obtained at a low speed, as will be seen by reference to the *annexed table;* and secondly, by means of an

alterable resistance placed in the circuit of the field-magnets the current and the speed of the dynamo can be kept constant whilst the resistance in circuit is altered, so that the number of baths in the circuit can be altered to suit our daily requirements without altering to any great extent the speed of the dynamo. This arrangement was therefore adopted, and a coiled iron wire resistance was made which allows of 1·2 ohm being introduced into the field-magnet circuit, proceeding by 0·06 ohm. As thus arranged, the resistance of the armature is 0·0297 ohm, and that of the field-magnet coils 0·684 ohm.

It is therefore evident that, so far as the dynamo is concerned, there is no reason why the baths should not be placed in series.

Accordingly arrangements were made to wind the solenoid of the governor so that the whole current of about 30 ampères would pass through it, the solenoid being placed in series with the baths. This was effected by winding the coil in two halves placed in parallel, the total resistance being 0·01 ohm, or rather more than that of one bath.

The governor was then fixed and adjusted by screwing up the spring until the ammeter showed a current of 30 ampères (nearly). As already mentioned, this governor has been found to answer admirably. At first the spring slackened slightly, and so reduced the current, but as soon as it had taken a set the current remained perfectly constant, with the exception of occasional rapid variations of small amplitude due to defects in the brushes.

Before beginning to deposit, it was necessary to ascertain which terminal of the dynamo should be connected to the receiving-plate. This information was obtained by observing the direction of the deflection caused by the dynamo on a galvanometer placed in a shunt-circuit, and comparing with the deflection produced on the same galvanometer by an ordinary cell.

A condemned plate was now placed in one of our ordinary horizontal baths and was deposited upon for 31¾ hours, the place of the remaining baths being taken by the coiled iron wire resistance above referred to. The deposited copper was removed, and found to be of excellent quality.

EXPERIMENTS WITH ELMORE DYNAMO.

Armature.—Diameter, 1′ 6½″. Arranged in two sets of 10 coils in series, the two sets being in parallel. Resistance cold, 0·0297 ohm.

Fields.—20 coils arranged in series. Resistance cold, 0·684 ohm. Placed in shunt with circuit.

No. of Experiment.	Revolutions per Minute. Dynamo.	Revolutions per Minute. Engine.	Steam Pressure at Engine. lbs. per square inch.	Electro-motive Force. Poggendorff's Method. Balancing Cells. E.M.F.	Balancing Cells. No.	Resistances. r_1	Resistances. r_2	Resistances. Of Leads to Dynamo	Diff. Potential Terminals of Dynamo.	Deduced from Current.	Mean.	Resistance in Main Leads.	Current. Cardew's Ammeter, R = 1,000 ohms. Constant, 18,280. Balancing Cells. E.M.F.	Balancing Cells. No.	Balancing Cells. Liquid Re-sistance.	Resistance Unplugged in Box of Coils.	Current in Main Leads.	Deduced from E.M.F. and Re-sistance.	Mean.
32	400	130	75	1·07	2	10	Not measured.	0·67	...	6·0	...	*0·33*	1·07	2	10	1,150	18·1	...	...
33	500	170	35	1·07	2	10	42·0	0·67	11·1	11·5	*11·3*	*0·38*	1·07	2	10	280	30·4	29·2	*29·8*
34	300	100	80	1·07	2	10	2·4	0·67	2·7	2·8	*2·75*	*0·34*	1·07	2	10	3,680	8·3	7·9	*8·1*
35	415	140	95	1·07	2	10	23·5	0·67	7·2	7·5	*7·35*	*0·34*	1·07	2	10	750	22·2	21·2	*21·7*
36	540	180	100	1·07	2	10	37·0	0·67	10·0	10·4	*10·2*	*0·36*	1·07	2	10	320	29·0	27·8	*28·4*
37	600	200	100	1·07	2	10	59·0	0·67	14·6	14·8	*14·7*	*0·41*	1·07	2	10	70	36·2	35·8	*36·0*
38	710	230	95	1·07	2	10	100·0	0·67	23·2	22·0	*22·6*	*0·51*	1·07	2	10	800	43·2	45·4	*44·3*

Remarks.—The engine was worked compound for all experiments, except No. 33, when it was worked simple. An additional resistance of 0·85 was placed in field-magnet circuit.

The weight deposited was 1,076 grammes, which corresponds to 28·7 ampères, using the latest determination of the electro-chemical eqvivalent of copper by Lord Rayleigh, viz., 0·003281. According to Cardew's galvanometer, the current during the deposition was 29·2 ampères.

It was observed that the back of the deposited copper was finer in grain than that deposited at the same rate by the Smee cells. It was thought that this apparent contradiction to the law that the quality of the copper depends on the rate of deposition could be accounted for on the supposition that the current from the Smee cells is variable, and this supposition is borne out by the fact that numerous bubbles of hydrogen are continually rising in these cells, but not by any means at a uniform rate, so that it appeared probable that the internal resistance and even the electro-motive force are continually varying. It was therefore surmised that deposits suitable for our purpose might be obtained with the dynamo, using a greater current strength than 30 ampères. To test this, the condemned plate above referred to was reduced in size so as to increase the density of the current, and the results obtained were as follow (the governor was re-adjusted and the current increased to 30·5 ampères.):—

Size of plate. Sq. ft.	Current per sq. ft.	Corresponding current on plates 2′ 4″ × 3′ 4″.	Resulting copper.
5	6·1	47·0	Good.
4	7·6	58·5	Good.
2·8	10·0	77·0	Good, rather rough at the edges.

Taking, therefore, the 2′ 4″ × 3′ 4″ plates as a standard, it appears that the current can be increased to at least 50 ampères.

Since the above was written, some experiments have been made to ascertain whether there is any variation in the current from the Smee cells, but no appreciable oscillation in the current or in the electro-motive force could be detected, only a gradual and very slight diminution. This matter therefore appears to require further research.

These deposits were made in a *vertical* bath. In the first experiment (on the plate of 5 square feet) the solution was intentionally allowed to remain undisturbed. The resistance gradually increased, but the governor kept the current constant until the throttle-valve was completely open, when of course the current began to fall off.

An india-rubber tube was now inserted in the bath, and air blown in by means of a small foot-bellows; the large bubbles of air in rising displaced the solution and very soon equalised its intensity. It is therefore hoped that it will be possible to use vertical baths, at any rate for matrices, the advantages gained being simplicity in construction and less space occupied than with horizontal baths.

Whilst the above experiments were being carried out, a matrix was laid down in one of the horizontal baths, with the intention of producing a duplicate sufficiently thick to print from; this intention was, however, not carried out, as it was found that without doubt the dynamo could produce as good copper as the batteries; and on the 10th December, 1884, Colonel Stotherd, R.E., Director-General of the Ordnance Survey, authorised the batteries being replaced by the dynamo. The experiments were thus brought to a close, and preparations are now being made for the contemplated change.

Postscript to Paper on Electrotyping with a Dynamo-electric Machine at the Ordnance Survey Office.

In the discussion on the paper Prof. Forbes questioned, and Mr. Crompton advocated the arrangement of the baths in series as applied generally to electrotyping operations. It is to be observed that, in the case of the electrotyping at the Ordnance Survey Office, copper of a *special* quality is required, and that therefore the regulation of the current is the paramount consideration; and in such a case, when the size of the plates to be deposited upon does not greatly vary, there can be no doubt that the series arrangement is the best, perhaps the only one. The current must be regulated directly, and not by regulating the volts, as in the case of incandescent lighting.

It does not, however, at all follow that the series arrangement is the best in all cases. For instance, in electro-plating, where goods of all sizes and shapes have to be deposited upon, and the density of the current (within certain limits) is of secondary consideration, the arrangement in parallel is to be preferred. The question raised in the discussion was, however, in connection with depositing copper in large quantities, and on this question the following calculations are submitted for consideration :—

It is assumed that the texture of the copper is not of the utmost importance, so that the *accurate* regulation of the current does not enter into the case ; at the same time such a regulation can only be of advantage.

Supposing that it were required to construct a dynamo capable of depositing 26 lbs. of copper per hour, assuming the following data:

Anodes and cathodes, 10 sq. ft. each, and 2 in. apart.

Resistance between an anode and corresponding cathode, 0·02 ohm.

Counter E.M.F., 0·05 volt.

Density of current, 10 ampères per sq. ft.

Now, whatever arrangement be adopted, the current between a pair of plates will be $10 \times 10 = 100$ ampères, and consequently the weight of copper deposited on each cathode in one hour is $\frac{100}{384} = 0{\cdot}26$ lbs., so that the number of pairs of plates required is $\frac{26}{0{\cdot}26} = 100$.

Arrangement in Parallel.

The current required in the mains will be $100 \times 100 = 10{,}000$ ampères, and to carry this current 10 leads of copper rod, each 1·13 inch diameter, are needed. One rod could, however, be dropped every 10th anode. The resistance of these leads, supposing them 200 feet long, will be found to be 0·00034 ohm, and that of the depositing baths 0·0002 ; total 0·00054 ohm. Hence

$$\text{Diff. pot. terminals of dynamo} = 5{\cdot}4 + 0{\cdot}05.^{*}$$
$$= 5{\cdot}45 \text{ volts.}$$
$$\text{Watts in mains} \quad \ldots \quad \ldots = 54{,}500.$$

* Counter E.M.F.

Arrangements in Series.

In this case the current in the mains will be 100 ampères, and if the leads are supposed to be 300 feet long, and made of 0·5 inch copper rod, the resistance in circuit will be—

Baths.		Leads.		Ohms.
2·0	+	0·025	=	2·025

Hence

Diff. pot. terminals of dynamo = 202·5 + 5·0.*
= 207·5 volts.
Watts in mains ... ... = 20,750.

Arrangements in Combined Parallel and Series.

Supposing 10 anodes and cathodes arranged in parallel, and these groups arranged in series, it will be found that a current of 1,000 ampères and a difference of potential at the terminals of the dynamo of 22·6 volts are required, and that the watts in the mains = 22,600. Similarly it will be found that, if 5 pairs of plates are arranged in parallel, thus forming 20 groups in series, 500 amperes and 43·1 volts are required.

From the above it appears that the arrangement in parallel is (electrically) the least economical, and the reason is that the resistance of the leads is, proportionately to that of the baths, much greater in this case than in the others. To reduce this loss to the same amount as in the arrangement in series would require leads of 1,360 square inches area at the dynamo, gradually reducing to 136 square inches. It is also to be observed that the least defect in the connections of the leads with the dynamo would reduce the output to an enormous extent. Thus the arrangement in parallel, apart from the question as to whether a dynamo producing a current of 10,000 ampères could at present be easily constructed, does not appear to be a practical arrangement.

The arrangement in series is the most economical, but the number of separate baths *insulated* from each other and the number of connections (three to each bath) might be considered excessive. Moreover, it was assumed that the plates were of

---

* *Counter E.M.F.*

equal area. Should this not be the case, the arrangement in series would not be easily applied; for if the strength of the current were adjusted for the largest plate, the density of the current would be too great for the smaller plates.

The disadvantages, both of the arrangement in parallel and of that in series, can be got rid of by some *combined* arrangement (not necessarily either of those mentioned above); and, although these deductions of course only apply strictly to the case of depositing 26 lbs. per hour, it is thought that in many cases a combined arrangement will be found practically the best.

List of Articles shown at the Meeting.

Samples of copper deposited by Messrs. Elmore.

Samples of copper deposited during experiments mentioned in paper, with current densities of 3·6, 4·34, 6·1, 7·6, and 10·0 ampères per square foot.

Matrix, of one inch, new series, part of London.

Willans' electrical governor.

Standard Daniell cell, plaster of Paris.*

Daniell cell, with two porous pots.

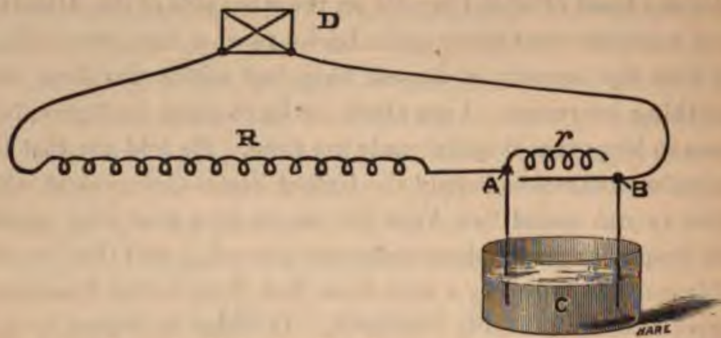
The President: We are much indebted to Captain Sankey for his very interesting paper; and, as I think it is the first time that the subject has been before the Society, I hope we may have it discussed very fully. There is one thing that Captain Sankey referred to that struck me, and that was with regard to the purity of the plate to be decomposed. In the Paris Exhibition of 1881 I saw some plates that had been deposited by a Siemens machine. These plates were about 4 feet by 3 feet, and about 1½ inches thick, and were of pure copper, and certainly they were most beautiful specimens of pure copper. Close to these were displayed the dynamo machines from which these plates had been made; and I remember the armatures of these machines, instead of being wrapped with coils of wire, were simply bars of copper about half an inch to three-quarters of an inch square. While I was looking at them, Mr. Wallace, from America, who did a large amount of work

* *These cells* can be obtained from Messrs. Casella.

in electrotyping, was with me, and very much admired the specimens of copper. But he told me that, without wishing to make any boast of what they did on the other side of the Atlantic, these machines were really quite babies to what they were using, and that the amount of deposit they had within the hour was something enormous. I am afraid really to quote his figure, but it was so large that it quite made me stare. He told me that he was under contract to supply the United States Government with a wire to run round New York (it was to be a steel wire coated with copper, to give 4 ohms resistance per mile), and that he was under contract to supply a wire from New York to San Francisco, to give 2 ohms per mile resistance. In order to convey to my mind the rate at which this deposit was put on the wire, he said that this wire continually flowed through the bath without intermission at a moderately slow speed, and that the deposit was even and well put on. If any gentleman present has had any experience on this subject, and can give us any information, I am sure we shall be very pleased to hear him.

Mr. H. Crookes: I should like to say a few words about some experiments I made a few months ago, in depositing copper on a small scale. The dynamo at my disposal was a rather powerful one, giving about 150 volts. The first experiment I tried was by connecting the cell in series with a high resistance to the dynamo; and although I had enough resistance to reduce the E.M.F. to about ·7 volt, the deposit was powdery, and could be brushed off with the finger, although it was made slowly. I next tried another method, which I can best explain by sketching it on the blackboard. D is the dynamo, R is a resistance of German silver wire absorbing 148 volts, r is a rheostat with a sliding-contact, so that all or any part can be put in circuit at will—it absorbs the other two volts; C is the depositing cell, through which flows a small current shunted off the main at A and B. It is obvious that if the whole of r is in circuit we get a difference of potential of two volts at the two sides of the cell, whereas if only half r is in circuit we get one volt in the cell, and so on; and as r is of low resistance, and is parallel to the cell, only a small current passes through the cell. With this arrangement I got very good

results, the best being with about ·65 volts, when the copper deposited was quite malleable. It may be of interest to some to



know that I used a Ruhmkorff coil connected in the same manner, the series method being well known to be of no use.

Professor Forbes: I should like to ask Captain Sankey one question. He told us that after some previous experiments they came to the conclusion that they ought to deposit their copper in series, instead of in parallel. Now, it is a very interesting point, and we all know that a number of people have been working to make dynamos with enormous quantity; and, for my own part, I have been feeling rather proud, I must confess, of having introduced a dynamo which gave a larger current than had ever been produced before, viz., 3,000 ampères; and, in some way, most of us have been under the impression that for depositing-work a very large current and small resistance were required. Now, Captain Sankey tells us that, from his own experience, we ought to put all the cells into series. You, Sir, in speaking from the chair, have told us about the experiments of Messrs. Siemens with their large enormous-quantity machine, and I would simply ask the question of Captain Sankey, whether those plates which were shown in the Paris Exhibition of 1881, which many of us visited, could have been so produced, or whether he has actually seen in practice copper produced, on his plan, at the same rate as it was produced by those very large "quantity" machines. [Captain Sankey replied in the negative.] I would further wish to know whether it is a mistake to try and make large "quantity" machines *for depositing.* Certainly it has always been thought up to the

present time that if we were to deposit a ton of copper a day, or anything like that, we required a large "quantity" machine, and that the disadvantage of having depositing-baths in series is very great; but the disadvantage is not so apparent in Captain Sankey's application of it as it would be in the case of a copper refiner's application of it, because all that Captain Sankey wanted to get was a good deposit for his printing-plate, whereas a refiner of copper wants to get as large a mass of pure copper as he can, and these are probably very different cases, and very likely my question is utterly irrelevant.

You, Sir, have also made some remarks about Professor Wallace's practice in America, and from what you said I suspect that you are unaware of the sad fate that has come over those works. The contract, I regret to say, just before I left America last summer, had, I heard, absolutely fallen through. The money which had been expended upon it by Professor Wallace was utterly lost, and the whole of those enormous lead tanks and gigantic dynamo machines, which you, Sir, were perfectly right in saying were simply eclipsing the Siemens machines, or any deposit of copper which had ever been produced in the world before—the whole of these dynamo machines and the whole of these tanks are simply being sold for old lead and old wire. I met one of the gentlemen who had been with Professor Wallace in connection with the whole of this work, in Philadelphia, and he almost cried when he described to me the condition of affairs. It seemed to me such a sad thing that so splendid a piece of work had fallen through.

Mr. W. H. Walenn confirmed Professor Forbes' report regarding the failure of the American Company, whose contract for coating telegraph wires according to certain conditions had fallen through. Certain experiments under his instructions had been carried on, resulting in the production of an adherent and soft coating of copper on the wire, which meets the requirements at once, without wanting annealing afterwards. He expressed surprise that Captain Sankey had got so exquisite a deposit from the ordinary electro-coppering solution (one pound of cupric sulphate and *one pound of sulphuric* acid to the gallon of water).

Doubtless this was due to the discovery Captain Sankey had made of arranging the troughs in series; not that this had not been done before, but that, to Mr. Walenn's knowledge, it had not been done with a view to get the best deposit. The ordinary electro-coppering solution gives usually an intractable botryoidal deposit, somewhat as if the copper were laid on parallel to the surface to be coated, like mortar on to a brick. Mr. Walenn had found (see *Philosophical Magazine* [4], xli., 41, January, 1871) that the deposit was improved by the addition of one ounce of zinc sulphate to the gallon, as recommenced by Napier; also that (see Specification of Patent No. 3,930, A.D. 1868) the further addition of a few drops per gallon of carbon disulphide ensured the deposit being vertical to the surface, instead of being parallel thereto.

Mr. Crompton: This subject is destined to become an extremely important one to the new industry of the manufacture of dynamo-electric machinery, which will owe a great deal to Captain Sankey, for he is the first person who has given us information on electro-deposition of metals. I, in common with many other makers of dynamo machines, have been very anxious to gather practical information on this subject. We want answers to the very questions which have been put this evening—as to the advantages of using dynamos giving enormous currents and extremely low electro-motive force over those of more ordinary construction. In Wales, in the smelting district, where an enormous quantity of copper is being dealt with, there are one or two large works where slabs of refined copper are obtained by electro-deposition, and the pure copper so obtained has been largely used by us for the winding of our dynamo machines. I, in common with others, have wished to manufacture dynamo machines suitable for this purpose, but I have never been able to obtain any information as to what their requirements really are. They did not know the difference between an ampère and a volt. They knew they had a machine which gave a good deal of trouble with the brushes, and that they would like something better. I, in common with the Chairman, have seen and admired the magnificent machines of Messrs. Siemens in the Paris Exhibition, and wondered at the very special machines then shown; but I

have never understood why a more ordinary type of machine could not be used, and I have been delighted to hear from Captain Sankey to-night that a machine with higher E.M.F. has been successfully used. To me it appears perfectly evident that a machine such as he describes, of higher electro-motive force, if it is placed any distance from the room where the baths are placed, must require far less costly connections and conductors than the machine of large currents and low E.M.F., and the further the machine is placed from the baths the more this will be felt. I hope Captain Sankey will put us still further in his debt if, when the process is carried out on a larger scale, as he has told us it is about to be, he will give us another paper with additional quantitative results; and I think if he chooses to avail himself of the more improved appliances that we can now give him, he will not have any more of the trouble he has described with the brushes of the machine, or from overheating and such-like matters, which are now well understood and have been successfully dealt with.

Mr. Adams: I beg to ask whether, under the system of currents in series, polarisation is caused to any considerable extent, and whether the resistance of the bath is deranged thereby?

Professor Ayrton: The author of the paper says that he found that 100 watts were necessary to deposit 1 lb. of copper per hour, though he mentioned that Messrs. Munro and Jamieson spoke of 305 watts. But of course the number of watts required to deposit any particular quantity of copper depended mainly on the resistance of charge in the electrolytic bath. The opposing electro-motive force here mentioned is only ·016 volt, whereas the actual number of volts employed was 9 volts, so that the mere opposing electro-motive force in the bath is hardly worth considering. Now, nearly the whole of the work done in an electrotyping or electro-plating bath is done on overcoming the resistance of the various baths and is spent producing heat; hence, of course, if the plates are very near together, as it appears they were in this case, the waste of power in heating is very small, so that, as Captain Sankey has told us, only 100 watts are required to deposit 1 lb. of copper per hour. If the plates could have been made still larger and nearer,

a less number of watts would be required, and therefore it is not surprising that, with plates probably smaller and further apart, Messrs. Munro and Jamieson found 305 watts were required. I have no doubt that when the baths are made with larger plates Captain Sankey will require to employ only from 60 to 70 watts.

Mr. D. C. BATE: I would ask Captain Sankey if I am correct in understanding that the resistance of the armature was ·0297 ω. If so, ·684 ω seems very low for the fields. I have endeavoured to listen to the various speakers whilst calculating, and may therefore be in error. As far as I can see, the external resistance was 2·63 ω, including leads and baths; there was, therefore, over 3·8 times more current in the fields than in the external circuit, that is about 29 ampères in the external circuit, and something like 100 through the fields, necessitating a much higher E.M.F. than stated. However, even with 11 volts the current exceeded 15 ampères. Sir William Thomson's rule makes the correct external resistance about 14 ω, or the square root of the sum of armature and shunt resistance. Professor Silvanus Thompson's rule, that there should be 400 times the resistance of the armature in the fields, makes over 11 ω as the correct resistance of shunt to get the best result from the dynamo.

Note.—In conversation with Captain Sankey after the meeting, I learned that there was an additional resistance of ·85 ω in the shunt circuit, which of course rendered his statement as to the current through it approximately correct.

Mr. KAPP: There is one point I should like to bring before the meeting with a view to clear it up, that is the question of counter electro-motive force. It seems to me that the whole success of depositing by means of machines turns simply on this question.

We can reduce the influence of resistance in the leads by putting the baths in series and thus have a very small loss, but counter electro-motive force is a thing we cannot get over, and the figures of Captain Sankey are so extremely low that I should be delighted to have them confirmed. I have made some experiments by placing ten cells in series,—only small cells,—*containing* a copper cylinder inside and a copper cylinder sur-

rounding it, filled with sulphate of copper which was kept at saturation by having a little copper basket on the top of the small cylinder, which was kept filled with crystals of sulphate of copper; and I first measured the resistance of these ten cells coupled up in series, reversing the currents through a Wheatstone bridge, and found it to be a certain figure—I forget what. Then I sent currents through it varying from 1 to 15 and up to 20 ampères, and measured the potential at the terminal of this row of cells. The method of doing this was rather rough, and the results, therefore, did not agree very well. One reason for this may be that the density of currents varied between wide limits, and the counter electro-motive force varied from ·07 to ·09 volt.

Mr. Roberts: It would be very interesting, I think, if Professor Forbes could tell us whether the failure of the Ansonia Company was due to their being unable to deposit the coating of copper on the steel core at a cost which would enable them to sell the wire for telegraph purposes, or if the failure of the company was due to any other difficulty connected with the manufacture of the compound wire. I am aware that this is not, strictly speaking, the question under discussion; but as reference has been made to the extensive apparatus the Ansonia Company had for depositing copper on iron wire, I may perhaps be allowed to remark that when Mr. Preece, about fifteen months since, read a paper at the Institution of Civil Engineers, on "Electrical Conductors," a gentleman who was interested in this company exhibited some samples of their compound wire. The copper was, I think, about 14 gauge, and the steel core about 18 gauge, and we were told that it was No. 4 wire drawn down to that gauge—that is, on a No 8 steel core, copper was deposited until the diameter was increased to No. 4 B.W.G., and it was then drawn down to the size I have mentioned. A good deal of doubt was expressed at the meeting as to whether it was possible to prepare a wire in that way, viz., to draw down a No. 4 copper wire with a steel core to No. 14 or 15, owing to the process of annealing copper and annealing steel being totally different. Copper, as all members present are aware, is annealed by its being heated and plunged in cold water, and steel by its being heated *and cooled slowly*. The Ansonia Company's

representative at the time told us that the wire was so drawn, and that it was annealed during the drawing, but he was not able to tell us how it was done; and I am afraid that nobody was really quite convinced that it was done. Perhaps Professor Forbes can tell us whether the failure of the company was due to any difficulty of this sort, or to the cost of depositing the copper on the iron core.

Professor FORBES: I can answer for it that that was not the case.

Professor AYRTON: I should like to answer one question. It is difficult to see how any such electro-motive force as ·9 volt would be produced in a cell consisting of sulphate of copper and two similar plates of copper, because the current multiplied by the electro-motive force measures the energy stored up per second in the cell in some form of chemical action. Now there is, practically, no energy stored up in an electrotyping cell. You start with copper, sulphate of copper, and copper, and end with the same amount of each of the same substances. In fact, the work done in sending a current in any cell may be divided into two parts—first, the work required to overcome the resistance and spent in heating the cell; secondly, the work expended in producing chemical action, as in the accumulator; but if you take two plates of the same material, and end with that same substance, then the energy would have disappeared, and therefore it seems almost certain that the opposing electro-motive force must be extremely small. I find, also, mentioned by Mr. Sprague a very great confirmation of what Captain Sankey has said. Mr. Sprague says, in his treatise, that the E.M.F. is ·02 volt; Captain Sankey finds it ·016, almost the same. If, however, the action is carried on so rapidly that water is decomposed, and hydrogen and oxygen are liberated, then the opposing electro-motive force may be much greater than ·02 volt.

Mr. PREECE: I think I can answer the question that was last put, as to the cause of the failure of the company in America to carry out this large process of depositing copper on a steel core. I was at Philadelphia with Professor Forbes, and at the Philadelphia Exhibition there was a very fine display of this compound wire,—various sizes, some very large, others rather small,—and there was a very intelligent American gentleman exhibiting this

display. He told me, not knowing me, in reply to my enquiries as to whether the process was largely used in America, that the company belonged to the Postal Telegraph Company, which was in the hands of Mr. Mackay; further, that the baths were all being destroyed because Mr. Mackay had been persuaded by some one in England that thin wires were better than thick wires, and that Mr. Edison disagrees with that person. I told him that I was that man, and that I had pointed out to Mr. Mackay that he could get precisely the same strength and precisely the same conductivity with hard-drawn copper of much smaller size than he got with this compound of steel and soft copper on the outside; and the result is that Mr. Mackay, like most of the telegraph companies in America, is now doing as we are in England—using hard-drawn copper, without damaging it with the interposition of a steel core or anything else.

I may also add that there is a very great distinction to be drawn between copper deposited for commercial purposes and copper deposited on plates for engraving in the mode in which engraving is carried out at the Ordnance Office in Southampton. The copper manufacturers are anxious to deposit copper, not in ounces, or in pounds, but in *tons* per week; and it was only last week that one of the leading wire manufacturers in this country said he wanted a machine that would deposit copper at the rate of twenty tons a week. Now, at Southampton they require to produce these very large plates for their maps, and I do not know that in all my experience I have ever seen a prettier operation carried out than that of depositing copper slowly, uniformly, and quietly in this way that you see here, so as to take up every delicate line and every delicate tracing on the original matrix. This is a matrix which Captain Sankey uses on a small plate, and it is necessary to deposit the copper here so regularly, and so equally and perfectly, that there shall be nothing but the very smoothest and the very clearest surface; and I recommend every one of you after the meeting to come and examine these plates for yourselves. You will find, not only that these lines are most faithfully produced on the face, but that they are actually carried through *the whole exterior of the* copper, and appear at the back

as well. Therefore I say this, that there is no hope for manufacturers using Mr. Crompton's machines, because they want to deposit copper at the rate of tons per week; and there is no hope for Professor Forbes, on the other hand, getting his enormous machine into use at Southampton, for there they want to do what has been done quietly and gently by the aid of small cell machines. But each has his proper field to work in, and there is room for both.

Capt. SANKEY: With reference to Mr. Crookes' experiments with a dynamo, I think the reason the copper was deposited in powder in the first instance is that, the E.M.F. being 100 volts, he must have got a great density of current. I have just roughly worked out what the density of the current was in Mr. Crookes' second experiment, that shown on the board, and it appears to be about 4 ampères per square foot. Now this plate here was deposited with 4·34 ampères per square foot, so that the reason he obtained good copper in the second experiment is very evident.

As regards the question of depositing in series or in parallel, raised by Prof. Forbes, that question has been so ably answered by Mr. Preece that I think there is nothing more for me to say. I may, however, add that the electrotyping, as practised at the Ordnance Survey, is rather different from what is done in the trade, and it is also to be remembered that electrotyping is quite different from electro-plating. In the case of the latter only a thin coating of metal is required, which is moreover generally burnished; but in our case the deposited plates have to be $\frac{1}{8}$ in. in thickness, and it is necessary the copper should be of a uniform quality. Therefore it is necessary that the current should be perfectly constant, and for the reasons given in the paper it is thought that putting the baths in series conduces to a constant current far better than putting them in parallel.

As regards the solution question asked by Mr. Walenn, the solution we use is the ordinary acidulated sulphate of copper solution. It is made up thus:—10 sulphate of copper, 30 water, and and 1 sulphuric acid by weight; but is then only used to dilute the solution in the baths, which, through evaporation, becomes too *dense*, and when it reaches about a density of 1,180 it is thinned

down by the solution thus prepared. I do not profess to say that it is the best solution for electrotyping, but it has been used at Southampton for years. I propose to make experiments with various solutions ultimately.

As to the point raised by Mr. Crompton, of the distance the baths could be placed from the machine, I may mention that the dynamo machine we have was, as originally arranged, supposed to be able to work a bath forty feet away, and the leads had to be one inch diameter of copper. As the dynamo is at present arranged, the farthest bath could be placed 100 yards or more away from the dynamo, using leads of seven strands of No. 12 B.W.G. copper, without considerable loss.

As regards the resistance due to polarisation, from what I gather from Mr. Sprague (as stated in Munro and Jamieson's "Pocket-book"), the polarisation is due to the difference in density of the solution against the two plates: near the dissolving-plate it will be denser than near the receiving-plate. In our baths, owing to the rocking motion, these densities are probably nearly equalised, and this would account for the low counter electromotive force in these baths (0·005 to 0·016).

Mr. Bates questions the proportion between the resistance of the armature and that of the fields. No doubt the proportion does not agree with the rules he gave; but it should be remembered that this dynamo was not constructed for a shunt dynamo: it was one that had to be altered, and it was not possible to make the proportion between the resistance of the armature and that of the fields correct. As to the current in the fields, I think he said 150 ampères. He must have made a mistake in his calculation: it really is about 9.

I may add that I have made preparations to carry out a series of experiments, which I hope to commence shortly, in electrotyping with copper, to find out really what kinds of copper are deposited with different densities of currents; and, in response to Mr. Crompton's request, shall be very pleased to place the results before this Society at some future meeting.

The following paper was then read:—

THE WORKING OF RAILWAY SIGNALS AND POINTS BY ELECTRO-MAGNETS, AND CONTROLLING THEM, IN CONJUNCTION WITH A COMPLETE BLOCK SYSTEM, EFFICIENTLY AND ECONOMICALLY BY A CURRENT FROM A PRIMARY OR SECONDARY BATTERY.*

By Illius A. Timmis, M. Inst. M.E.

The discussion on the foregoing paper was adjourned until Thursday, the 26th February, 1885, and the paper and discussion will appear in the next number of the Journal.

A ballot then took place, at which the following were elected:—

Member:

A. F. Matveieff.

Associates:

Thomas Newton Andrews.
Charles Butler Clay.
James Law Crawford.
Alan Danvers.
Ernest Danvers.
E. T. Dawson.
Frank E. Elmore.
William C. C. Hawtayne.
Sydney F. Huxley.
George Nichol.
Captain H. R. Sankey, R.E.
George William Selby.
William Henry Snell.

Student:

Chas. A. Baker.

THE LIBRARY.

ACCESSIONS TO THE LIBRARY TO FEB. 20, 1885.

BY ALFRED J. FROST, *Librarian.*

(Works marked thus (*) have been purchased.)

(Works marked thus (†) were presented by JOHN AYLMER, Esq., *Member, Local Hon. Sec. for Paris.)*

IT IS PARTICULARLY REQUESTED THAT MEMBERS WILL PRESENT COPIES OF THEIR WORKS TO THE LIBRARY AS SOON AS POSSIBLE AFTER PUBLICATION.

† **Academie des Sciences.** Coups de foudre observés en France pendant le second semestre de l'année 1883. 4to. 17 pp. [Ext. des *Comptes Rendus des Séances de l'Academie des Sciences*, t. XCVIII. Séance du 7 Avril, 1884.] *Paris*, 1884

† —— Coups de foudre observés en France pendant le premier semestre de l'année 1883. 4to. 15 pp. [Ext. des *Comptes Rendus des Séances de l'Académie des Sciences*, t. XCVIII. Séance du 11 Février, 1884.] *Paris*, 1884

* **Allard** [E.] Mémoire sur l'intensité et la portée des phares. 8vo. 118 pp. *Paris*, 1876

* —— Note sur quelques objections relatives à l'emploi de la lumière électrique dans les phares. 8vo. 16 pp. [Ext. des *Annales des Ponts et Chaussées, Numero de Mai*, 1882.] *Paris*, 1882

American Bell Telephone Co. v. The People's Telephone Co. In Equity. Opinion of Judge Wallace. 8vo. 40 pp. *New York*, 1884
[Presented by the American Bell Telephone Co.]

† **Baile** [J. B.] Détermination de l'Ohm. Étude de la méthode de l'amortissement des aimants. 8vo. 41 pp. [Ext. des *Annales Télégraphiques*, Mars-Avril, 1884.] *Paris*, 1884

* **Baudouin** [F.] Observations sur le mode d'établissement des Lignes Télégraphiques sous-marins. 8vo. 31 pp. *Paris*, 1858

† **Baudouin et Digney Freres.** Perfectionnements apportés à la transmission automatique des signaux télégraphiques de Morse. 8vo. 8 pp. Plate. [Ext. du *Bulletin de la Presse Scientifique.*] *Paris*, 1859

Bell. [*Vide* Prescott.]

Benoit [*Vide* Mascart, De Nerville, et Benoit.]

Berly [J. A.] Universal Electrical Directory and Advertiser. The Electrician's "Vade Mecum," containing a complete record of all the industries connected directly or indirectly with Electricity and Magnetism, and the names and addresses of manufacturers in England, etc. 8vo. 217 + 223 + 113. *London*, 1885

Blackwood [Wm.] Electricity in Abdominal Disorders. 8vo. 6 pp. [The *Medical Bulletin. Vol. VI. No. 12.* Dec.] *Philadelphia*, 1884

† **Blavier** [E. E.] Etude des Courants Telluriques. 4to. 30 pp. *Paris*, 1884

Bosscha. [*Vide* Raynaud.]

Churchill [John F.], M.D. Stœchiological Medicine, and its application to diseases of the lungs and other organs. 8vo. 23 pp. *London*, 1884

† **Cabanellas** [Gustave]. Transport électrique de la puissance mécanique. 8vo. 58 pp. [Soc. des Ingénieurs Civils. 16 Fev., 2 et 16 Mars, and 20 Avril, 1883.] *Paris*, 1883

Cew. [*Vide* Glaser-de-Cew.]

Clark [Latimer]. Transit Tables for 1885. Giving the Greenwich mean time of transit of the sun, and of certain stars, for every day in the year, with an ephemeris of the sun, moon, and planets. 8vo. 71 pp. *London*, 1885

† **Conférence Internationale pour la détermination des Unités Electriques.** Procès-Verbaux. 16th Oct.—26th Oct., 1882. Fo. 163 pp. *Paris*, 1882

† —— [Proceedings.] Deuxième Session. Fo. 121 pp. *Paris*, 1884

† —— Deuxième Session. Deuxième Commission Électricité Atmospherique et Courants Terrestres. Fo. 36 pp. *Paris*, 1884

Day. [*Vide* Royal Engineers.]

† **Delaurier** [Emile.] Notice analytique des Inventions de M. Delaurier. 8vo. 20 pp. *Paris*, 1881

† —— Notice des appareils de M. E. Delaurier exposés dans la Réunion de la Société Physique les 27 et 28 Mars, 1883. 8vo. 5 pp. *Paris*, 1883

† —— Essai d'une théorie générale supérieure de philosophie naturelle et de thermo-chimie. 8vo. 82 pp. *Paris*, 1883

De Cew. [*Vide* Glaser-de-Cew.]

De Nerville. [*Vide* Nerville.]

† **Digney Frères.** Télégraphie à cadran imprimant les dépêches en caractères ordinaires imagine et construit par MM. Digney Frères. Rapport fait à la Soc. d'Encouragement pour l'industrie Nationale, par M. le Vicomte Th. Du Moncel. 8vo. 28 pp. *Paris*, 1859

† —— [*Vide* Baudouin et Digney Frères.]

* **Dredge** [James] [Editor]. Electric Illumination. By James Dredge, Dr. M. F. O'Reilly, and H. Vivarez. With Abstracts of Specifications having reference to Electric Lighting, prepared by W. Lloyd Wise. Vol. II. 4to. 455 + cccxciv. + xiii. pp. *London*, 1885

† **Du Moncel** [Comte Th.] Détermination des éléments de construction des Électro-aimants suivant les applications auxquelles on veut les soumettre. 8vo. 39 pp. *Paris*, 1874

† —— Note sur le Magnétisme. 4to. 7 pp. [Ext. des *Comptes Rendus des Séances de l'Academie des Sciences*, t. LXXX. Séance du 1er Mars, 1875.] *Paris*, 1875

† —— Deuxième Note sur les Électro-aimants tubulaires a noyaux multiples. 4to. 6 pp. [Ext. des *Comptes Rendus des Séances de l'Académie des Sciences*, t. LXXXI. Séance du 5 Juillet, 1875.] *Paris*, 1875

† —— Du role de la terre dans les transmissions télégraphiques. 8vo. 62 pp. *Caen*, 1876

† —— Les Pierres Conductrices. 4to. 6 pp. [Ext. *L'Electricité*, 1re année. No. 1. 15 Janvier, 1876.] *Paris*, 1876

—— [*Vide Digney* Frères.]

† **Eddy** [A. H.] The Mather Dynamo-electric Machine, for Plating and Electrotyping. 8vo. 21 pp. *Hartford, Conn.*, 1882

Exposition International d'Electricite, Paris, 1881. [*Vide* Conférence International pour les détermination des Unités Electriques.]

Fischer-Treuenfeld. [*Vide* Treuenfeld.]

Fahie [J. A.] Electric Lighting considered from a sanitary point of view. 8vo. 8 pp. Vol. VI. [*Trans. of the Sanitary Institute of Great Britain.* Read Oct. 3, 1884.] *London*, 1884

* **Glaser-de-Cew.** Magneto- and Dynamo-electric Machines, with a description of Electric Accumulators. Translated from the German by F. Krohn, and specially edited, with many additions, by Paget Higgs, LL.D. [*The Specialists' Series*, Vol. I.] *London*, 1884

Higgs. [*Vide* Glaser-de-Cew.]

Houston [Prof. J. E.] Primers of Electricity. 8vo.
No. 1. Arc Lights. 2. Incandescent Lights. 3. Dynamo-electric Machines. 4. Voltaic Batteries. 5. Electric Circuits. 6. Effects of an Electric Current. 7. Magnetism. 8. Electro-Magnetism. 9. Electricity produced by Friction. 10. The Telephone. 11. The Microphone. 12. The Storage Battery. 13. The Electric Telegraph. 14. Electro-Metallurgy. 15. Electric Motors. 16. The Transmission of Energy. 17. The Lightning Rod. 18. The Galvanometer. 19. Electric Measurements of Minute Times. 20. Electric Clocks. 21. Synchronous Multiplex Telegraphy 22. Electric Arc Lamps. 23. The Ruhmkorff Induction Coil. 24. The Jablochkoff Candle. 25. Sources of Electricity. *Philadelphia*, 1884

Iron and Steel Institute. Journal. No. 2, 1884. 8vo. 746 + lxv. pp. With List of Members. *London*, 1884
[By Exchange.]

Jablochkoff. [*Vide* Houston.]
——— [*Vide* Société Générale d'Électricité.]

† **Jarriant** [B.] Étude sur les paratonnerres. Des paratonnerres à longues tiges, et leurs avantages. 8vo 51 pp. *Paris*, 1884

Jewesbury [H. W.] A Short History of the Introduction of Gutta Percha into Europe. 8vo. 15 pp. *London*, 1883

Jurgens [C.] Nozioni elementari di Elettricità e Magnetismo. 8vo. 104 pp. *Roma*, 1885

Kirchhoff. [*Vide* Raynaud.]

Krohn. [*Vide* Glaser-de-Cew.]

* **Le Tual** [A.] Étude du Télégraphie automatique de Sir Ch. Wheatstone. 8vo. 289 pp. and Atlas of xlviii. plates. *Paris*, 1876

† **Ludewig** [J.] Gewitterbeobachtungen im Reichs—Telegraphengebiete. 4to. 11 pp. [Separatabdruck aus der *Elektrotechnischen Zeitschrift*, Marz, 1884.] *Berlin*, 1884

Lui. [*Vide* Sélégman—Lui et Tongas.]

* **Lupton** [S.], M.A. Numerical Tables and Constants in Elementary Science. 12mo. 96 pp. *London*, 1884

† **Madsen** [C. L.] Recherches sur la loi du Mouvement Télégraphique International. 8vo. 68 pp. *Copenhague and Paris*, 1877

† **Magne** [Ch.] Notice sur les paratonnerres, leur origine, leur construction, et leur installation. *8vo. 31 pp.* Paris, 1882 (?)

† **Maiche** [L.] Notice sur l'origine des transmetteurs téléphoniques et microphoniques. 4to. 20 pp. *Paris*, 1882

† **Mascart** [M.] Sir William Siemens. 4to. 7 pp. [Soc. d'Encouragement pour l'Industrie Nationale.] *Paris*, 1884

† **Mascart** [M.], **De Nerville** [F.], **et Benoit.** Résumé d'expériences sur la détermination de l'ohm et de sa valeur en colonne mercurielle. La. 8vo. 71 pp. *Paris*, 1884

† **Mourlon** [C.] Système de Télégraphie et de Téléphonie Simultanées par les mêmes fils, de F. van Rysselberge. 8vo. 35 pp. *Bruxelles*, 1884

Munro [J.] Electricity and its Uses. 8vo. 180 pp. *London*, 1883

Nerville [F. De]. [*Vide* Mascart, De Nerville, et Benoit.]

O'Reilly [Dr. M. F.] [*Vide* Dredge.]

† **Park** [Roswell], M.D. The Electric Light in Surgical Diagnosis. 8vo. 22 pp. [Reprinted from the *Annals of Anatomy and Surgery*, March, 1883.] *Brooklyn, N.Y.*, 1883

† —— Secondary Batteries and the so-called Storage of Electricity. 8vo. 16 pp. [Reprinted from the *Chicago Medical Journal and Examiner*, February, 1883.] *Chicago*, 1883

Pasqualini. [*Vide* Ròiti and Pasqualini.]

Prescott [Geo. B.] Dynamo-Electricity: its generation, application, transmission, storage, and measurement. 8vo. 867 pp. *New York*, 1884

—— Bell's Electric Speaking Telephone: its invention, construction, application, modification, and history. 8vo. 526 pp. *New York*, 1884

† **Raynaud** [J.] De lois de la propagation de l'électricité dans l'état permanent. 8vo. 17 pp. [Ext. du *Journal de Physique*, t. I. 1872.] *Paris*, 1873

† —— Courants dérivés; lois de Kirchhoff. 8vo. 12 pp. [Ext. du *Journal de Physique*, t. II. 1873.] *Paris*, 1873

† —— Résolution des équations fournies par les lois de Kirchhoff, pour la distribution des courants électriques dans un système quelconque de conducteurs linéaires. 8vo. 11 pp. [Ext. du *Journal de Physique*, t. II. 1873.] *Paris*, 1873

† —— Courants dérivés. Corollaires de M. Bosscha et applications. 8vo. 14 pp. [Ext. du *Journal de Physique*, t. II. 1873.] *Paris*, 1873

† —— Des appareils employés pour mesurer les résistances électriques. 8vo. 5 pp. [Ext. du *Journal de Physique*, t. II. 1873.] *Paris*, 1873.

† —— Détails pratiques sur la mesure des résistances électriques. 8vo. 7 pp. [Ext. du *Journal de Physique*, t. II. 1873.] *Paris*, 1873

† **Ròiti** [Prof. A.] **and Pasqualini** [Col. Dott. Luigi.] Osservazioni continue della Elettricitæ atmosferica istituite a Firenze. 4to. 10 pp. Plates. [R. Ist. di Studi Pratici e di Perfezionamento in Firenze.] *Firenze*, 1884

Royal Engineers. Professional Papers of. Edited by Captain Francis J. Day, R.E. Occasional Papers. Royal Engineers' Institute. Vol. IX. 1883. 8vo. 217 pp. *London*, 1884 [By Exchange.]

Ruhmkorff. [*Vide* Houston.]

Rysselberghe. [*Vide* Mourlon.]

* **Seligman—Lui et Tongas.** Notice sur les essais électriques des lignes télégraphiques. 8vo. 36 pp. [Ext. des *Annales Télégraphiques*, May-Juin, Juillet-Août, 1881.] *Paris*, 1881

Siemens [Sir W.] [*Vide* Mascart.]

† **Siemens et Halske.** Proposition d'une unité de lumiére, et remarques sur l'execution des unités électriques. 4to. 9 pp. *Berlin*, 1884

Société d'Encouragement pour l'Industrie Nationale. [*Vide* Digney Fréres.]

† **Société des Electriciens.** Comité d'Organisation. 8vo. 31 pp. *Paris*, 1883

† **Société Générale d'Électricité (Procedés Jablochkoff).** Eclairage par la bougie électrique des voies publiques des Grands Magasins, etc. 8vo. 40 pp. *Paris*, 1880

Society of Arts. Index to Journal. Vols. XXI. to XXX. (1872 to 1882.) 8vo. 109 pp. *London*, 1884

† **"Times, The."** [Dec. 3rd, 4th, 5th, 7th and 8th, 1868.] Discussion: Telegraph to India. 8vo. 16 pp. *London*, 1868

Tongas. [*Vide* Séligman—Lui et Tongas.]

Treuenfeld [R. v. Fischer.] Die Kriegstelegraphie in den neueren Feldzügen Englands, Afghanistan, Zululand, Aegypten. 8vo. 98 pp. Plates. *Berlin*, 1884

† **Trouvé** [G.] Solution pratique de l'éclairage électrique domestique. 8vo 24 pp. *Paris*, 1883

† —— L'Électricité au théatre. Bijoux électro-mobiles. Nouveaux bijoux électriques lumineux. 8vo. 30 pp. *Paris*, 1884

Tual. [*Vide* Le Tual.]

Vivarez [H.] [*Vide* Dredge, J.]

Wise [W. L.] [*Vide* Dredge.]

† **Varley** [Cromwell F.] Description of the Translating Apparatus and Universal Galvanometer. 8vo. 12 pp. *London*, 1863

† **Varley** [S. A.] Railway Train Intercommunication. 8vo. 20 pp. [Soc. of Engineers, 9th June, 1873.] *London*, 1873

† **Violle** [M. J.] Note sur les expériences effectuées pour la determination de l'étalon absolu de lumière. 8vo. 20 pp. *Paris*, 1884

Wheatstone. [*Vide* Le Tual.]

† **Wiedemann** [G.] Versuche zur Bestimmung des Ohm. 8vo. 6 pp. *Leipsig*, 1884

* **Young** [Arthur]. Voyage en France pendant les années] 1787-88-89-90, éduit aux parties de l'agriculture et de la statistique 8vo. 520 pp. *Paris*, 1792 (?)

Zetzsche [Dr. K. E.] Die Entwickelung der Automatischen Telegraphie. 8vo. 65 pp. *Berlin*, 1875

[Presented by R. von Fischer-Treuenfeld, Esq., Member.]

ABSTRACTS.

J. B. BAILLE—MEASUREMENT OF THE HORIZONTAL COMPONENT OF THE EARTH'S MAGNETISM BY THE METHOD OF DAMPING.

(*Comptes Rendus, T.* 99, *No.* 17, *p.* 704, *October* 27, 1884.)

It is known that a magnetised bar swinging inside a coil of wire experiences a totally different degree of damping accordingly as the circuit of the coil is opened or closed. By measuring the damping in the two cases, the difference will give the moment, h, of the force exerted by the induced currents on the magnet. If M is the magnetic moment of the bar, R the resistance in ohms of the coil, and G the galvanometer constant expressed in centimètres, then

$$M^2 = R\,G^2\,h\,;$$

and as the product, H M, can be readily determined by the method of oscillations, knowing the moment of inertia of the magnet, H can be readily found. The advantages of the method are, the rapidity with which it can be carried out, hence H is determined for the exact moment of the experiment; and the fact that only one magnet need be used.

There are two sources of error which require to be eliminated: self-induction, which can be got rid of by taking a coil with only a few turns of wire, and especially by attaching to the magnet a body having a large moment of inertia, so that the time of the swing may be long. Another source is the variation in the declination, which tends to render the damping unsymmetrical, and appears as an irregularity in the oscillations. It can be avoided by taking care to make the observations rapidly and to deal with an even number of amplitudes.

The author has obtained very good results by his method, from which the following are selected; the experiments having been made at the magnetic observatory of Villeneuve-St.-Georges, near Paris, and the values being given in the C.G.S. system:—

1884.	Magnets.	Gauss's method.	Damping method.
September 25	A	0·19430	0·19381
"	B	0·19413	0·19532
September 26	M	0·19487	0·19512
... ...	N	0·19450	0·19464

C. M. GARIEL—CAUSE OF DEATH FROM VIOLENT ELECTRICAL SHOCKS

(L'Electricien, Vol. VIII, No. 82, p. 193, September 1, 1884.)

The author, together with Professor Brouardel, investigated the causes of the death of two persons on the occasion of the Press fête on the evening of the 6th August, 1882, in the Gardens of the Tuileries, which were temporarily illuminated by electricity. One of the circuits consisted of twelve lamps worked in series by an alternating current. The leads for this circuit—one of which was insulated, the other bare—were carried for part of the distance along a wall forming one side of a ditch, the other side of which was a grass slope. Two young men, in trying to cross this ditch, came in contact with these wires, and one was killed instantaneously; the other expired in the course of a few minutes.

The *mean* difference of potential between the two wires was probably about 500 volts, but as they were traversed by an alternating current, the maximum electro-motive force was much greater. The difference of potential between either wire and the earth may be put at 250 volts.

One of the men had slight burns on both hands, and it is presumed that he must therefore have touched the bare wire at two points. The burns were slight, and would have been caused, probably, by the breaking contact when the hands were torn away from the wire by the body falling. The brain was very congested on its surface, and the heart was entirely empty. Dr. Bourrot, who made the autopsy, concluded that death was due to stoppage of the heart's action, which was brought about by a violent shock to the pneumogastric nerve, the one which runs from the brain to the heart. The stoppage of the heart's action was not instantaneous at the moment of the accident, but the heart was unable to resume its pulsations, and this lead to asphyxia, which explains the congested state of the brain.

In the case of the second man, whose death was instantaneous, slight burns were found on one hand and on the side of the face and the ear. There was some slight alteration in the substance of the brain, which, however, in this case was not at all congested. There were numerous extravasations of blood in the pleuræ of the lungs, and the heart was found full of blood, which, it was especially remarked, was of an exceptionally brilliant colour. Death in this case was due to instantaneous stoppage of the heart's action.

The ground at the scene of the accident was dry, and there seems no reason to suppose that a current passed through the men from one wire to earth. In proof of this a witness was examined, who stated that he also had been in the ditch; that he had touched the bare wire with one hand; that his arm had been violently thrown back; but that he suffered no inconvenience whatever from the slight shock he experienced. The burns found on the two bodies clearly indicate that the two unfortunate men had been in contact with the wire at *two* points, and the author suggests, therefore, that their death was the ultimate result of the passage of a shunt current through part of their bodies. The intensity of such a shunt current could only have been a very small fraction

of the whole current, for the resistance offered by the body to the passage of the current must have been enormous in comparison with that of a copper wire six millimètres in diameter and a few decimètres long; but it is far from being proved that, in the action of electricity on organised beings, it is the intensity of the current which can be taken as a measure of the effects produced. It is more probable that the difference of potential is the cause; or perhaps the intensity of the action is due to the energy, *i.e.*, to the product of the current by the electro-motive force. In this connection it may be remarked that a somewhat similar theory exists as an explanation of burns. The gravity of a burn depends much more on the temperature of the body, which would correspond to potential, than on the quantity of heat. During the period of contact, however short, of the body with two points of a wire carrying an alternating current, the body must have followed the variations of potential occurring in the wire, and its potential must have passed very rapidly through very widely different values. The electricity acted indirectly by partly suspending the action of the nervous system, from which suspension death resulted.

V. BABLON—MECHANICAL DEPOLARISATION OF BATTERIES.

(*L'Electricien, Vol. VIII., No.* 82, *p.* 207, *September* 1, 1884.)

The positive electrode is constructed of a thin pliable metallic band, stretched like a strap on two pulleys, one of which is placed at the bottom of the cell, the other on the top, out of the liquid. On rotating the upper pulley slowly, the metal band will be put in movement, and will be alternately immersed in the liquid and exposed to the air. The depolarisation may be assisted by arranging brushes to rub against the band as it circulates, so as to wipe off the greater part of the adherent liquid. In a Daniell cell, the circulating electrode would be of thin sheet copper, while three zincs could be used in the cell, one between the two parts of the copper band, and one on either side. The circulating band may be arranged either vertically or horizontally.

J. LAFFARGUE—MODIFICATION OF THOMSON'S BRIDGE ARRANGEMENT FOR VERY SMALL RESISTANCES.

(*L'Electricien, Vol. VIII., No.* 84, *p.* 307, *October* 1, 1884.)

In Thomson's original method, balance is brought about by shifting two points of contact on a stretched wire. The author objects that it is not possible to obtain a standard wire homogeneous throughout its length, and that though the variations from one part to another of the wire may be intrinsically small, yet they are relatively large as compared to the small resistances to be measured.

His modification is shown in the figure on the next page.

The general equation for Thomson's bridge is

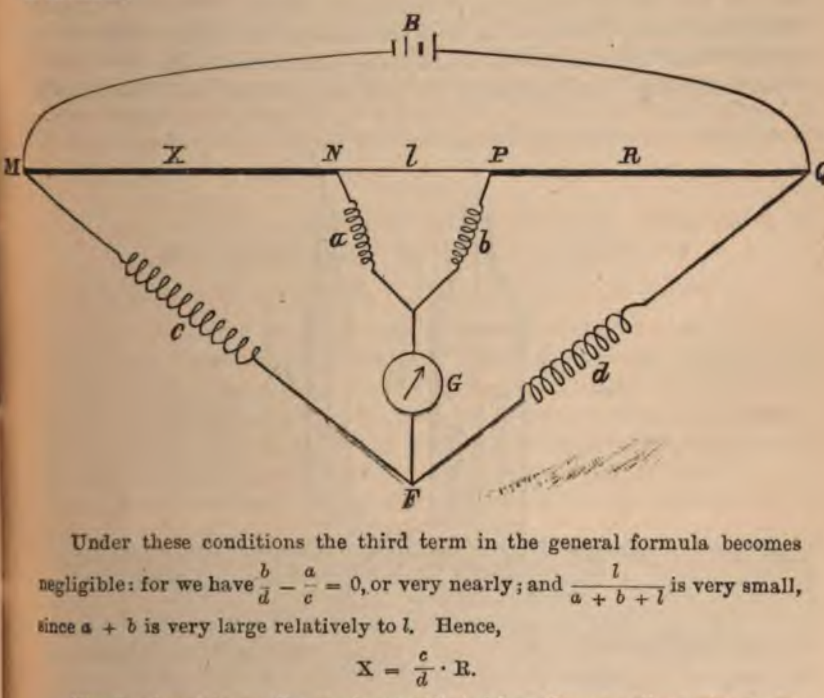
$$\frac{R}{a} - \frac{X}{c} + \left(\frac{b}{a} - \frac{a}{c}\right) \times \frac{l}{a + b + l} = 0.$$

The method of using the arrangement illustrated is the following:—

In the first place the ratio $\frac{a}{b}$ is made equal to unity; then the galvanometer is brought to zero by altering the resistances c and d. The resistances a and b are then arranged so as to satisfy as nearly as possible the equation

$$\frac{b}{d} - \frac{c}{a} = 0.$$

The galvanometer is again brought to zero by adjusting c and d, and a fresh approximation is then made. Practically, after two trials the above equation is satisfied.



Under these conditions the third term in the general formula becomes negligible: for we have $\frac{b}{d} - \frac{a}{c} = 0$, or very nearly; and $\frac{l}{a+b+l}$ is very small, since $a + b$ is very large relatively to l. Hence,

$$X = \frac{c}{d} \cdot R.$$

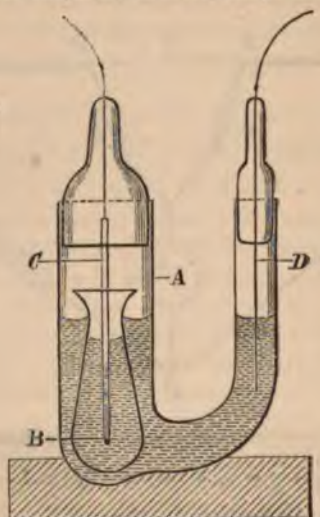
The fixed resistance, R, may be constituted by a German silver wire 5 mm. diameter, and of one-tenth of an ohm resistance.

CROVA and GARBE—AN ELECTROSTATIC STANDARD OF POTENTIAL.

(*Journal de Physique, T.* 3, *p.* 299, *July*, 1884.)

The authors were dissatisfied with the form of cell, zinc and platinum in pure water, which they were using for some electrometer measurements, owing to the want of good insulation and to the polarisation of the platinum. They therefore constructed a special new form of cell, of which they give the very

best report with respect to the above two points. The accompanying sketch shows the cell in its natural size. It consists of a glass U tube, A, with limbs of unequal size, seated in a block of ebonite, and with stoppers formed of closed pieces of glass tubing ground to fit into the two necks. The electrodes are sealed into these glass stoppers, and the whole external surface is varnished. B is a small tube of potash glass, which is placed in the larger limb and kept upright by small protuberances blown on it. The outer tube, A, is filled with a solution of copper sulphate; the inner tube, B, with a solution of zinc sulphate. C is a rod of zinc soldered to a platinum wire, and D is a copper wire. It will be seen that the level of the liquid in B is below that in A, and that the two liquids can never communicate directly. Electrical contact takes place through the layer of moisture formed on the surface of B. In a modified form the liquids were replaced by pumice stone soaked in them. Instead of using copper sulphate, the cell may be used with bichromate



solution and a platinum wire, the zinc and zinc sulphate solution remaining unchanged.

The authors state that they will publish the exact constants of their cells later, but they have had them in use long enough to make sure of their constancy as regards electro-motive force, even after being short-circuited for several hours.

E. REYNIER—STANDARD CELL FOR MEASUREMENT OF ELECTRO-MOTIVE FORCE.

(*Journal de Physique, T. 3, p. 448, October*, 1884.)

The form of Daniell cell which the author proposes to use has a positive electrode of thin sheet copper bent into corrugations, and placed cylindrically *in a glass vessel.* The negative electrode is a zinc rod, 3 mm. in diameter, in

the centre of the jar. The copper has an effective surface of thirty square decimètres, the whole of it being immersed in the solution. The total capacity of the cell is 800 cubic centimètres, and its resistance varies from 0·2 ohm to 4 ohms, according to the liquid used. The electro-motive force naturally varies with the liquid used, and the author experimented with several. With ordinary zinc and solution of zinc sulphate, it is just one volt. In spite of the advantage of this value, the author prefers not to use zinc sulphate, which is always more or less acid, but takes a solution of sea salt, with which and with an amalgamated zinc he obtained the value 0·82 volt between 5° C. and 40° C. The resistance in this case is from 1 to 2 ohms. Short-circuited through 820 ohms, the cell only lost one per cent. of its electro-motive force in an hour. The constancy of the cell may be easily explained. When the current is very weak, the formation of the products of oxidisation, by the action of the air, on the very large surface of the copper, surpasses the reducing action of electrolysis and keeps the electro-motive force up to its maximum value. The very trifling quantity of zinc dissolved cannot appreciably alter the constitution of the large mass of liquid used.

M. DEPREZ—TRANSMISSION DYNAMOMETER.

(La Lumière Electrique, T. 13, No. 39, September 27, 1884, p. 481.)

The motive force which has to be measured is applied to a loose pulley on the shaft of the motor; this pulley bears a stud which presses against a flat spring joined to the shaft, exactly as in Poncelet's dynamometer. It is the bending of this spring which has to be transmitted to a registering pencil, which should not be affected by the rotation of the loose pulley; this is accomplished by means of an epicycloidal gearing, consisting of six toothed wheels mounted two and two on three axles. The two wheels on each shaft are of

different sizes, but the three larger wheels and the three smaller wheels have the same size respectively. The first pair is placed on a sleeve with a bore sufficiently large to pass over any ordinary-sized shaft, the sleeve being fixed to the shaft by means of three set screws. The larger of the two wheels is loose on the sleeve, and is joined up to the loose pulley on the shaft of the motor; the smaller wheel is rigidly fixed to the sleeve. Both wheels of the intermediate *pair are loose on their* axle, and gear on one side into the first

pair described above, and on the other into the third pair, which are rigidly united. The foregoing sketch shows more clearly the epicycloidal gear.

The central axle is supported by two arms, ending in friction-rings on the sleeve of the first pair. Two connecting-rods unite the central and third axles. From the figure it will be seen that the whole connecting system is analogous to a pair of compasses which would tend to close if they were not supported at the joint. This support can be fixed at any height convenient for working the apparatus, and can be provided with a screw working in a nut, by means of which the whole gearing can be raised or lowered.

If the axle is not acted on by any tangential force, which corresponds to the case of the spring on the pulley exerting no tension, the first pair of wheels are driven at an equal rate of speed, which they transmit to the two other pairs, and the whole six wheels revolve simultaneously and without relative displacement; on the contrary, when the spring is bent under the action of a force acting on the axle, an angular displacement of the first pair of wheels is caused which is proportional to the tension of spring. This displacement is transmitted by the intermediate pair of wheels, which are loose on their axle, to the third pair, and would tend to break their rigid connection, if the line of centres of the second and third pair were not capable of relative motion. If we denote the angular displacement of the first pair by a_1, and that of the line of centres by a_3, these two angles are related by the equation

$$\frac{a_3}{a_1} = \frac{R \times r}{R^2 - r^2}$$

The movement of the connecting-rod can be magnified by means of the pointer and any form of parallel motion; and a continuous record of the work transmitted can be obtained by fixing a pencil to a point on this parallel motion and causing a strip of paper to pass below it.

A. KUNDT—THE ELECTRO-MAGNETIC ROTATION OF THE PLANE OF POLARISATION IN LIGHT IN IRON, COBALT, AND NICKEL.

(Annalen der Physik und Chemie, B. XXIII., H. 2, No. 10, pp. 228-252, 1884.)

This question has been investigated by Kerr and Gordon, but the amount of rotation having been very small, was difficult of measurement. The author has repeated and amplified Kerr's experiments, and confirms all his results, with one exception. He found a reversal in the direction of rotation when the light was reflected from the pole of a magnet under an incident angle of about 80°, if the plane of polarisation of the incident ray and the plane of incidence were at right angles. This rotation is contrary to Kerr's results.

The great novelty in the author's researches is that he has made use of transparent layers of the metals, and has thus been able to investigate the action of the magnetic field on *transmitted* light as well as on reflected light, and he has found that transparent layers of iron, cobalt, and nickel in the magnetic field have a very strong rotary power on the rays transmitted. The *rotation with iron for* rays of mean wave length is thirty thousand times as

great as with glass of the same thickness. The rotation in all three metals takes place in the same direction as that of the magnetising current.

For the experiments on reflection, small circular steel mirrors 35 mm. in diameter and 6 mm. thick were used; also mirrors of speculum metal plated with iron or nickel; and, finally, glass mirrors coated with very thin transparent layers of iron. At first the experiment was tried of depositing electrolytically iron on silvered glass mirrors, but unsuccessfully, as the silver was dissolved off the glass. Afterwards the author made use of glass mirrors on which a very thin transparent layer of platinum had been deposited by König's method, and he succeeded entirely in coating these platinum mirrors with any one of the three metals, iron, cobalt, or nickel, the layer being so thin that light was still easily transmitted through the two layers of platinum and the metal under examination.

For the experiments on the rotation of the rays transmitted through the three metals, one of Ruhmkorff's forms of electro-magnet was used. The exciting current was about 16 ampères, and was furnished by a Gramme dynamo machine. The plates to be experimented upon were placed between the poles of the magnet, at right angles to the direction of the rays, which were those of sunlight. First the amount of rotation due to the glass and platinum alone was measured, and then that due to the glass, platinum, and iron, nickel, or cobalt; and it was found that the effect of the platinum was practically nil. The table gives the results obtained; in all these cases the rotation of the plane of polarisation is in the same direction as the Ampèrian currents.

Iron No. 1, transmitted light brown	3° 30′
Iron No. 2 (very thin), transmitted very light brown	0° 54′
Iron No. 3	2° 6′
Iron No. 4, thin layer	1° 30′
Ditto thicker layer	3° 42′
Cobalt No. 1	1° 18′
Cobalt No. 2, very transparent	1° 4′
Cobalt No. 3, same plate as No. 2	1° 0′
Cobalt No. 4	1° 58′
Nickel No. 1	0° 39′
Nickel No. 2	0° 49′

The iron mirrors also produced a certain amount of dispersion: thus it was found that light which had passed through a deep red glass and then through the mirror was more strongly rotated than blue light obtained by passage through ammoniacal solution of copper oxide. Similar experiments with cobalt and nickel showed only very slight dispersion. The specific rotatory power of iron was determined with a mirror having a layer of iron only 0·000055 mm. thick, and was found to be 32,000 times as great as that of the glass used. Circular polarisation was also observed with iron; the difference of the refractive indices for the ordinary and the extraordinary rays in the direction of the axis of a quartz crystal has been determined by Lang as only 0·0000718, while for iron the author found the same difference to be 0·1.

In the experiments on light reflected at right angles the author found the rotation for all three metals to be in the opposite direction to the Ampèrian currents, and to vary in value from 20 minutes of arc to one degree, about. Further experiments were tried with light reflected at an angle, and it was found that the greatest rotation, about 9 minutes of arc, occurred at an angle of 65° for iron. When the plane of polarisation was parallel to the plane of incidence, the direction of rotation was the same as that of the molecular currents; when at right angles, a reversal took place at about 82°. For nickel mirrors the same held good, except that the reversal took place between 50° and 60°; the rotation is less than with iron, not exceeding 5 minutes of arc.

The following are the conclusions the author draws:—

1. Most isotropic solid bodies, liquids, and those gases at present experimented upon, rotate the plane of polarisation in the positive direction (that of the molecular currents of Ampère).

2. Concentrated solution of chloride of iron rotates negatively. The negative rotation of other magnetic salts is shown by the decrease in the positive rotation due to the dissolving medium.

3. Oxygen, which is comparatively magnetic, rotates positively.

4. The plane of polarisation of light which has been transmitted through iron, nickel, or cobalt is rotated positively.

5. If it has been reflected at right angles, the rotation is negative.

6. The complicated phenomena observed by reflection at an acute angle from the end or side of a magnet may be explained on the assumption that at reflection the light rays traverse a very thin layer of the iron, and that negative rotation occurs in this layer.

BOUDET—RESEARCHES ON THE CONDUCTIVITY OF RAREFIED AIR AND THE ELECTRICAL POLARISATION OF GLASS.

(*Beiblätter, B.* 8, *St.* 11, *No.* 11, *p.* 834, 1884.)

The author describes the well-known darkening in one place of the discharge in a Geissler's tube by an electrified point brought near to it, when, as is known, a dark space becomes apparent, and he mentions that this disappears if an oppositely charged point is brought near to the same spot. He further describes afresh the experiment, where a tube held in the hand and brought near the prime conductor of a statical machine glows on being touched with one electrode; he has repeated, also, the old experiment of Riess, that a discharge can be produced in a tube by touching it, if the electro-motive force directly available at the electrode is too weak. He refers afterwards to the phenomenon that an exhausted bulb (*e.g.*, a glow lamp) shows luminosity on friction, and after it has become dark it can again be rendered luminous by touching it. He has compared the charge of an exhausted bulb with that of one of the same size filled with gold-leaf or with water by means of the length of the spark on discharge after the two bulbs had been charged by an equal number of turns of the frictional machine. This charge is smaller for the gold-*leaf than for the empty* bulb, and smaller for the bulb filled with water than

for that filled with gold-leaf. Hence, on the passage of a current, a charge is developed on the surface of the glass of the discharge tubes the more readily the more the gas is rarified, and the more powerfully the better the conductivity of the outer surface of the glass.

If the platinum wires of a glow lamp are connected to earth, and a piece of tinfoil laid on the top of the bulb, and a spark allowed to pass to this tinfoil, the luminosity of the bulb is different, accordingly as the spark is positive or negative, and a positive or negative discharge issues from the inner side of the glass under the tinfoil.

H. F. WEBER—THE ABSOLUTE VALUE OF THE SIEMENS MERCURY UNIT, AND THE LENGTH OF THE MERCURY COLUMN EQUIVALENT TO AN OHM.

(*Beiblätter*, *B.* 8, *H.* 11, *No.* 11, *p.* 838, 1884.)

In the first place the author corrects the value obtained in 1876-7: according to this correction the mercury unit is equal to $0{\cdot}9520 \times 10^9 \frac{\text{cm.}}{\text{sec.}}$ (not 0·9550).

In 1880 and 1882 the author used the so-called Kirchhoff's method of observing the current induced in a second coil on making and breaking the circuit of another coil near it. One coil (A) was laid horizontally on the top of a stone pillar and the other (B) was supported co-axially just above it on three legs. Both coils were wound on bronze frames, A with 644 turns, and B with 643 turns of wire 1·85 mm. in diameter. The width of the coils was 4·30 and 4·31 cm. respectively, and the radius of the outside layer 17·618 and 17·670 cm. The induced current was measured by means of a galvanometer specially constructed for the purpose. The mean of the experiments of 1880 gave the mercury unit equal to 0·9498 ohm, and the mean for 1882, 0·9500 ohm, or about one-quarter per cent. less than the former determinations. The mean of 50 observations with a similar but larger apparatus in 1883 gave the mercury unit equal to $0{\cdot}9496 \times 10^9 \frac{\text{cm.}}{\text{sec.}}$, the highest and lowest values being 0·9509 and 0·9486. Hence, 1 ohm is the resistance at 0° C. of a column of mercury 1 mm.^{2} in section and 105·32 cm. long.

Dr. DORN—AVOIDANCE OF LOCAL MAGNETIC INFLUENCE IN MEASURING INSTRUMENTS.

(*Elektrotechnische Zeitschrift*, *B.* 5, *No.* 10, *p.* 403, *October*, 1884.)

The author draws attention to the way in which results may be falsified by the magnetic properties of especially the copper wire used for winding galvanometers or for dampers, as well as of other materials used in their construction. Thus he has found that white marble is diamagnetic, while grey marble is indifferent. The best way of getting over the difficulty in the case of copper wire, *etc.*, *is to use only copper* which has been electrolytically

deposited, and which should therefore be quite free from iron. Also the author recommends that the use of any metal in the immediate vicinity of the needle of any galvanometer should be as far as possible avoided. It is not only the permanent magnetism which has to be considered, but also the temporary magnetism produced by the magnets which are employed in the instrument.

L. WEBER—REPLY TO MÖLLER'S PROPOSAL FOR USE OF POLARISATION PHOTOMETERS.

(Elektrotechnische Zeitschrift, B. 6, No. 1, p. 24, Jan., 1885.)

The exactness of photometric measurements depends on (1) the judgment as to the equal brightness of two sources of light, or of two surfaces illuminated by them, and (2) on the measurement and calculation of the changes in intensity of the two sources of light. The former is the more difficult and beset with most sources of error. In most cases it is the sensibility of the human eye to light which is used as the means of comparison. The more nearly equal are the two lights, the larger they are within certain limits, and the nearer they are to each other, the less are the errors introduced. Bunsen's method is undoubtedly the safest and best. With a well-trained eye the error for a single measurement can scarcely be reduced below one per cent. A closer approximation may perhaps be hoped for by using indirect criteria.

The second class of errors, owing to changes in the intensity of the light may be more readily dealt with, and can be reduced below the above limit. All photometers which depend on the sensibility of the eye may be divided into two classes, those depending on the law of inverse square of the distance, and on polarisation. The kind of photometer to be used must depend upon the circumstances; and if Herr Möller has obtained exceedingly good results with polarisation photometers, this is due to the great care taken in the experiments, and it is more than probable that he would have reached equally good results with a Bunsen photometer. At any rate, he is not justified in stating that "only polarisation photometers can be used for exact measurement."

A. WASSMUTH—USE OF MAGNETIC SCREENS FOR RENDERING GALVANOMETERS ASTATIC AND HIGHLY SENSITIVE.

(Zeitschrift für Elektrotechnik, Vol. 2, No. 17, p. 514, September 15, 1884.)

Former steps in this direction have resulted in the use of an iron cylinder round the magnet needle, by which the directive action of the horizontal component of the earth's magnetism was reduced to one-tenth.

The author's method is applicable to any kind of galvanometer with a single needle, whether it works on a pivot or is suspended by a fibre. In the same plane as the needle is placed a permanent steel magnet, with its longitudinal axis in the prolongation of the axis of the needle, and with, say, its S. pole facing the S. pole of the needle. By moving this magnet nearer to or *further away from the* needle, a first approximation to sensitiveness is obtained.

So far, the method presents nothing new. Having adjusted the magnet as nearly as possible by hand, the author then brings on the far side of the magnet, and at right angles to it, a bar of soft iron; when, by sliding this bar in the direction of its length, the action of the magnet on the needle can be reinforced, and a still nearer adjustment made. Should this not suffice, a second bar of soft iron can be placed alongside the magnet, but so as to make an angle with it; by altering this angle the last adjustment may be made. In place of this side bar of iron, two or more bars may be used at the end of the permanent magnet.

An example will show how largely the sensitiveness of a galvanometer may be increased by this means. A constant current of $0{\cdot}355 \times 10^{-6}$ ampères was passed through a high-resistance (16,000 units) Siemens mirror galvanometer; and when the scale was at a distance of 2,689 mm., the deflection was 22·4 mm. The permanent magnet and successive bars of soft iron were then brought into position, and the deflection increased as shown below:—

	Distance from end of magnet to bar. cm.	Deflection. mm.
Needle without magnet	—	22·4
Magnet in position (21·5 cm. from needle)	—	297·5
Magnet and 1 bar	1·0	334·8
" "	0·5	458·0
" 2 bars	0·5	502·3
" 3 bars	0·5	529·3
" 1 bar (thicker)	0·5	534·0
" " and a cylinder of iron on bar...	0·5	873·5

so that the same current now caused a deflection thirty times as great as at first.

Since the above current gave a deflection of 873 mm., it could be calculated what current would give a deflection of 1 mm.; and the author concludes that this excessively small current, which could still be observed on the galvanometer, would take 845 years to decompose one milligram of water.

A thermo-couple of copper and iron, with a difference of temperature of only 1·8 degree between the ends, gave a deflection of 11 mm. through a total resistance of 3,000 Siemens units. Assuming with Becquerel that the electromotive force of such a couple for a difference of temperature of 100 degrees is equal to 0·0018 volt, we find that the current in this case was about 5×10^{-9} ampères.

The author has gone yet further, and with the same Siemens galvanometer has increased the sensitiveness 135 times, but leaves the description of this arrangement for a later paper.

J. WEBER—SOME POINTS IN THE SOURCE OF VOLTA-ELECTRICITY.

(*Zeitschrift für Elektrotechnik, Vol.* 2, *Nos.* 18 *and* 19, *September* 30 *and October* 15, 1884, *p.* 558-590.)

The points to which the author draws attention are such as may perhaps lead to a quicker realisation of a cheap and efficient form of primary battery, no great progress having been made in this direction for the last fifty years. The working of every form of battery depends, on the one hand, on the electrical difference between a difficultly oxidisable metal (in which class carbon must be included) and an easily oxidisable one (practically zinc); on the other hand, on the simultaneously developed electrical difference between these metals and the gaseous components of watery fluids. The atoms of hydrogen which tend towards the negative pole are the carriers of the undefined *something* which appears in the external circuit as an electric current. Two plates of one and the same metal (platinum) produce, however, a powerful current, after they have been used for a few moments for the electrolysis of water. The plates themselves remain chemically unaltered. Also a cell composed of two platinum plates and two antagonistic liquids will produce electricity. Two lead plates in dilute sulphuric acid will also give a powerful polarisation current under similar circumstances. The cause of the current must be the difference of potential between lead and lead peroxide. Grove has constructed a gas battery from two plates of platinum dipping into diluted sulphuric acid, and two gases, hydrogen and oxygen. Collecting together the above points, we have—

1. Two different metals, two liquids, of which one can be decomposed by a solid body. The electrode which becomes positively polarised is dissolved (oxidised), and sends out negative electricity into the circuit.

2. Two platinum plates, one liquid. The previously negatively-polarised plate, charged with hydrogen, sends out negative electricity. Neither electrode is oxidised.

3. Two platinum plates, one acid, one alkaline liquid. Neither electrode is oxidised. The one charged with oxygen produces negative electricity.

4. Two lead plates, one liquid. The previously negatively-polarised plate sends out negative electricity. An amorphous metallic oxide plays the part of the electro-negative metal of the second plate, not of the electrode covered with it.

5. Two platinum plates, partly immersed in liquid, two gases, which surround the unimmersed portions of the plates. Neither electrode is polarised, neither is oxidised. The one surrounded with hydrogen sends out negative electricity.

If now we consider electricity, like magnetism, as a special state of matter, then this state can be brought about by five different causes. If we call in the aid of the æther, we are not much advanced, for we must admit that a certain manifestation of force, which we call electricity, has no fixed material cause.

Since the electricity furnished by the five typical cells given above does

not differ in kind, but only in degree, if on comparison we can find some property which is common to all without any exception whatsoever, we may conclude, with a high degree of probability, that the existence of this property must be the cause of the production of the electric current. In fact, if we confine ourselves to the case of cells on closed circuit, we find that such a common property of all the five types is the existence of oxygen and hydrogen. With the exception therefore of the fifth type, we may say that the existence of free or nascent hydrogen and oxygen on the electrodes is common to all voltaic cells. The oxygen and hydrogen, being nascent, have a greater chemical affinity than the same gases in their ordinary condition, and they will therefore not remain for an instant free, if any elements with which they can combine are in their neighbourhood. In all voltaic cells, therefore, new molecules of water will be formed, and their formation is the ultimate result of a movement in a fixed direction, which, probably in the form of undulations, can take place, either from one electrode to the opposite, or from the centre towards both, or from both towards the centre. It is probable that this movement always takes the shortest path. Since this movement always occurs in voltaic cells, we must conclude that the production of the electric current is dependent upon it, *i.e.*, on the motion itself, and not on the formation of water, which is only a means to the end. The nature of the electrodes is indifferent so long as they are in a condition to conduct the electricity produced, and are not destroyed by the active gases. The so-called electro-positive metals owe their denomination exclusively to their capability of being oxidised, the electro-negative to their not being attacked by acids. Since the development of hydrogen and oxygen in the five typical cells arises in five different ways, it follows that the nature of this development of gas cannot be essential. The elementary conditions for the existence of an electric current are therefore—

1. The production of nascent oxygen and hydrogen at such points of an acidified liquid that a continuous movement is set up, which probably arises from the decomposition and recombination of the molecules of water, and proceeds either in one direction or in two opposite directions, and which results finally in newly formed water.

2. Two plates of metal (or carbon) which are in a condition to take up and propagate this movement.

The whole of our galvanic batteries, with the exception of the first type, are in reality gas batteries. From the elementary conditions enumerated above, it would appear that the conditions for the rational construction of an economic battery are—

1. The production of hydrogen and oxygen (perhaps also chlorine) outside the battery.

2. The introduction of these gases in the active (nascent) condition.

3. An acid liquid of the highest possible conductivity.

4. Electrodes of some material with very fine pores and least liable to be acted upon (probably only carbon would do), in the form of closed vessels with large surface, which should permit of the passage through the sides of the gases brought into them under pressure in the ratio of 1 : 2.

It is not possible to consider so uncertain an experiment as Volta's fundamental one as the explanation of a new theory—an experiment which can give no genuine results; for how can it be possible to bring two metal plates into contact with each other without friction, without pressure, without chemical action due to the surface layers of gas, without traces of moisture, and without differences of temperature? any one of which circumstances would suffice to explain the phenomena observed. To attribute to the various elements an individual and innate electrical polarity, in order to explain thereby the secret of chemical attraction, seems a mere play upon words; it is as much as to say, for instance, that hydrogen and oxygen attract each other because they possess attractive properties.

Dr. A. VON WALTENHOFEN—OLD AND NEW VALUES OF THE ELECTRO-MOTIVE FORCE OF A DANIELL'S CELL.

(*Zeitschrift für Elektrotechnik, Vol. 2, No. 23, p. 705.*)

The author himself, with a cell in which the zinc was immersed in sulphuric acid diluted in the proportion of 1 of acid to 15 of water, has found the value D = 1·088 volt. The values which are given in old text books are all less than this. Amongst these are the values found by Müller and by Poggendorff, which are expressed in arbitrary units; but, recalculated in absolute units, they come out 0·971 and 0·922 respectively. The next group of values were determined in absolute measure, but the determinations were made by Ohm's method, which, owing to polarisation, would lead to too small a value. They are—

Von Beetz	D = 0·936 volt.
„	D = 0·887 „
J. Regnauld...	D = 1·016 „
Buff	D = 1·054 „
Bosscha	D = 1·026 „

The author's determinations of the E.M.F. of the Daniell cell were the first which were made by Poggendorff's second compensation method, and also the first in which Siemens' original resistance apparatus was used.

H. F. Weber found the value D = 1·0954 volt, on the assumption that one Siemens' unit equals 0·956 ohm. Recalculating this value on the basis of the Congress ohm, which is equal to 1·06 Siemens' unit (or 1 S. U. = 0·943 ohm), we find D = 1·080 volt. Reynier has found D = 1·079 volt, and Alder Wright the same value. It is, however, to be noted that the last three experimenters used a solution of sulphate of zinc in the zinc cell.

Kittler has found the E.M.F. of a gravity form of Daniell without partition higher than that of a cell with a partition, 1·195 volt in the former case and 1·111 volt in the latter. The author's value, 1·088 volt, was arrived at from experiments with nine different cells, the internal resistance of which varied from 0·38 S. U. to 4·1 S. U.

LIST OF OTHER ARTICLES.

(*Comptes Rendus, Vol.* 99.)

No. 5.—**MASCART**—Absolute Value of H at Paris.

No. 9.—**C. DECHARME**—Comparison between Electro-chemical- and Thermal-coloured Rings.

No. 12.—**J. LUVINI**—Source of Atmospheric Electricity.

No. 13.—**G. TISSANDIER**—Second Experiment with Balloon propelled by Electricity. **G. CHICAUDARD**—Law of Electro-chemical Decompositions.

No. 14.—**TRESCA**—Experiments on Secondary Generators at Turin.

No. 15.—**E. DUCRETET**—New Astatic Galvanometer.

No. 16.—**A. DE TILLO**—Magnetic Observations made in Russia. **QUET**—Solar Induction. **MAZE**—Disruptive Discharge of Holtz Machine.

No. 17.—**DUPONCHEL**—Solar Energy and Compass Variation.

No. 18.—**G. LIPPMANN**—Condition of Equilibrium of a Liquid Sheet under the action of Electro-magnetic Forces.

No. 19.—**G. GAZEAUX**—Natural Forces and the Nature of Electricity.

No. 20.—**LE ROUX**—Demonstration of the Inversion of E.M.F. of an Iron-copper Couple at a High Temperature. **BENOIT**—Construction of Standard Copies of the Legal Ohm. **L. GREZEL**—Note on some Electrical Experiments.

(*L'Electricien, Vol.* 8.)

No. 83.—**G. CHAPERON**—Use of the Telephone in the Measurement of the Internal Resistance of Batteries. **E. HOSPITALIER**—Self-Induction.

No. 84.—**E. HOSPITALIER**—Self-Induction and Alternate-current Machines. Confusion between the Units of Force and Work.

No. 85.—**G. CHAPERON**—Electro-Dynamometers. **E. HOSPITALIER**—Graphic Method of Teaching Electricity.

No. 86.—**G. CHAPERON**—(See above). **E. HOSPITALIER**—Self-Induction and Continuous-current Machines. *Anon.*—Telegraphic Communication without Wires.

No. 87.—**D. MACNAB**—Effect of the Insulation of the Battery on Tests for Insulation Resistance. **H. FONTAINE** — Purification of Copper Electrolytically.

No. 88.—**G. CHAPERON**—Alternate-current Machines. **M. BRILLOUIN**—Capacity of a Condenser, and Vibrations of a Tuning Fork. **W. M. MOORSOM**—Self-Induction. **H. FONTAINE**—(See above).

(*L'Electricité, Vol.* 8.)

No. 11.—The Electric Light on the Battlefield. Motive Power absorbed in Arc Lamps. Use of Dry Piles as Accumulators.

No. 14.—Primary Batteries for Electric Lighting.

No. 15.—Rudder *for Scheel's Torpedo.*

(*Journal de Physique, Vol.* 3.)

June.—**A. CHERVET**—Distribution of Potential in Conductors through which Permanent Currents flow.

October.—**R. BLONDLOT**—Influence of the Electric Condition of a Liquid on the Maximum Surface Tension of its Vapour.

(*Journal Telegraphique, Vol.* 8.)

No. 11.—**ROTHEN**—Earth Currents. *Anon.*—Observations of Variations of the Needle in connection with Earthquakes.

(*La Lumière Electrique, Vol.* 13.)

No. 36.—**P. BARBIER**—The Galvanometer of Deprez-d'Arsonval for Practical Use. **JAN CANESTRELLI**—Calibration of Galvanometers.

No. 37.—**C. DECHARME**—Lightning Conductors of Amiens Cathedral.

No. 38.—**P. CLEMENCEAU**—New Safety Valve for Steam Boilers. **ROITI and PASQUALINI**—Atmospheric Electricity at Florence.

No. 39.—**W. DE FONVIELLE**—Steering Balloons by Electricity.

No. 40.—**G. BADIA**—Electrolytic Extraction of Copper from the Ore. **A. GRAVIER**—Working of Gramme Ring as an Electro-Magnet.

No. 42.—**A. ROUILLARD**—Method of Measuring the Fall of Potential in a Cable.

No. 43.—**P. CLEMENCEAU**—Papers relating to Incandescence Lighting. **F. GERALDY**—Jablochkoff's New Battery.

No. 44.—**A. ROUILLARD**—Method of Measuring Capacity of a Cable. **F. LARROQUE**—Lightning and Lightning Conductors.

(*Vol.* 14.)

No. 45.—**LEBLANC**—Note on Two Problems in Telephony. **G. RICHARD**—Some Details in the Construction of Dynamos. **R. GUERIN**—Electric Water-level Indicator.

No. 46.—**MARIE DAVY**—Electricity at the Mont Souris Observatory. **G. RICHARD**—(See above). **F. LARROQUE**—Telephonic Reproduction of Sounds by the Electro-magnetic Vibrations of the Air. **C. C. SOULAGES**—The Log of Captain Fleuriais.

No. 47.—**M. H. MANGON**—Steering Balloon of MM. Renard & Krebs. *Anon.*—Neumeyer's Telephone.

No. 48.—**G. SZAVARDY**—Systems of Absolute Units. **F. UPPENBORN**—New Method of Measuring very Small Resistances

No. 49.—**G. SZAVARDY**—(See above).

No. 50.—**M. DEPREZ**—Direct Reading Ampère Meter. **G. SZAVARDY**—(See above). **G. VINCENTINI**—Conductivity of very Dilute Saline Liquids.

(Annalen der Physik und Chemie, Vol. 23.)

No. 1.—**H. HERTZ**—Relation between Maxwell's Electro-dynamic Fundamental Equations and those of the Opposed Theory.

No. 2.—**F. H. KOOSEN**—Depolarising Action of the Current in Batteries.

No. 3.—**W. VON BEZOLD**—Researches on Dielectrical Discharges and Conductivity. **C. L. WEBER**—Galvanic Conductivity and Thermo-electric Position of Amalgams. **F. NEESEN**—Influence of Magnetisation on the Resistance of Magnetic Fluids. **W. VOIGT**—Theory of the Rotation of Plane of Polarisation.

(Beiblätter, Vol. 8.)

No. 10.—**N. SLOUGUINOFF.**—Compensation Method for Measurement of E.M.F. **A. GUEBHARD**—E.M.F. of Derivatives of Lead Peroxide. **BELTRAMI**—Poisson's Theory of Magnetic Induction. **C. H. C. GRIMWIS**—Equation of Motion of an Electro-magnetic Field in connection with Maxwell's Theory.

No. 11.—**C. L. MENGES**—Measuring Instruments for Technical Use.

(Centralblatt für Elektrotechnik, Vol. 6.)

No. 25.—**A. HOTTENROTH** — Source of Atmospheric Electricity. **K. STRECKER**—Reproduction of Siemens' Unit.

No. 27.—**W. THOMSON**—Electrical Units.

No. 28.—*Anon.*—Compound and Shunt Dynamos.

No. 29.—**A. VON ETTINGHAUSEN**—Clark's Cell and its Use for Calibration. **EDLUND**—Electricity in Rarefied Air.

No. 30.—**ROITI**—Determination of the Resistance of a Wire in Absolute Units.

No. 31.—**F. UPPENBORN**—Distribution of Electrical Energy.

No. 32.—*Anon.*—Contact Breaker for Large Currents. **HUMMEL**—Current and Tension Meters.

No. 33.—**Dr. V. WIETLISBACH**—Theory of the Telephone. **F. E. NIPHER**—Representation of Resistance by a Velocity.

No. 34.—**E. BÖTTCHER**—Electrotechnical Properties of Nickel. **EDLUND**—Dimensions of the Unipolar Induction of the Earth.

No. 35.—**G. PLANTÉ**—Globular Lightning.

(Elektrotechnische Zeitschrift, Vol. 5.)

No. 8.—**A. PERENYI**—Methods of determining the Emission of Heat from, and the Rise of Temperature of Electric Conductors. **Dr. DIETRICH**—The Claims of Glow Lamps. *Anon.*—Striking Distance of Sparks.

No. 9.—**Dr. A. VOLLER**—Wolff's Dry Pile. **W. MÖLLER**—Photometric Methods. *Anon.*—Microphone with Alternate Currents.

No. 10.—*Anon.*—Siliceous Bronze Wires. **W. MÖLLER**—(See above). **W. VOLLBRECHT**—New Form of Resistance for use in Measuring Large Currents.

No. 12.—**Dr. FRÖLICH**—Siemens' New Machines for Electro-Metallurgy and Refining. **Dr. H. ARON**—A New Electricity Meter.

(*Exner's Repertorium, Vol.* 20.)

No. 10.—**Dr. H. KRUSS**—New Bunsen Photometer. **H. BECQUEREL**—New Method for Measuring Current in Absolute Units.

No. 11.—**EDLUND**—Dimensions of the Earth's Unipolar Induction. **F. E. NIPHER**—Representation of Resistance by a Velocity.

No. 12.—**HURION**—Changes in the Physical Properties of Bismuth in a Magnetic Field.

(*Zeitschrift für Elektrotechnik, Vol.* 2.)

No. 16.—**A. VON ETTINGSHAUSEN**—Clark's Cell.

No. 17.—**W. P. HAUCK**—New Battery.

No. 20.—*Anon.*—Lead-covered Cables. *Anon.*—Electric Separation of Iron and Zinc Ores.

No. 21.—**R. VON FISCHER-TREUENFELD**—The New Field Telegraph Organisation. **Prof. TENNOR**—Researches on a Theory of Electricity.

No. 22.—**O. VOLKMER**—Telpherage. **R. VON FISCHER-TREUENFELD**—(See above). **T. SCHWARTZE**—Secondary Batteries.

No. 23.—**J. PECHAN**—Determination of the Size of Wire for Dynamo Coils. **Dr. H. HAMMERL**—New Way of Winding Gramme Ring. **R. VON FISCHER-TREUENFELD**—(See above).

JOURNAL

OF THE

SOCIETY OF

Telegraph-Engineers and Electricians.

Founded 1871. *Incorporated* 1883.

VOL. XIV. 1885. No. 56.

The One Hundred and Forty-first Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 26th, 1885—C. E. SPAGNOLETTI, Esq., M. Inst. C.E., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The following transfers from Associates to Members were announced:—

George B. Almond.
John Scudamore Sellon.
J. R. H. Williamson.

Donations to the Library of the Society were announced as having been received since the last meeting from the following:—C. Jurgens, of Rome, and the Institution of Civil Engineers, to whom a vote of thanks was accorded.

The balance-sheet having been presented, according to the Articles of Association, the PRESIDENT moved that it be received and adopted. Mr. W. E. LANGDON seconded the resolution, which was thereupon carried.

The PRESIDENT stated that, in accordance with the minutes that have been read, the resolution of the Council in reference to the Electric Lighting Act has been sent to the Board of Trade, and that we have had an official acknowledgment of its receipt.

On the occasion of our last meeting, the discussion of Captain Sankey's paper occupied rather longer than we anticipated, and there was only sufficient time left for Mr. Timmis to read his paper, the discussion on which we shall therefore take to-night. I am glad to see many railway gentlemen here, whose valuable opinions we shall be very pleased to hear, as the system of signal-working on all railways is a most important one, and, as regards its cost, a very expensive item since the introduction of the locking-system.

[The following paper, although read at the last meeting, is here inserted, instead of in the previous number of the Journal, so that the paper and discussion may appear together.]

THE WORKING OF RAILWAY SIGNALS AND POINTS BY ELECTRO-MAGNETS, AND CONTROLLING THEM, IN CONJUNCTION WITH A COMPLETE BLOCK SYSTEM, EFFICIENTLY AND ECONOMICALLY BY A CURRENT FROM A PRIMARY OR SECONDARY BATTERY.

By Illius A. Timmis, M. Inst. M.E.

The objects of this paper are to describe—

1st. The new long-pull electro-magnets, the invention of Mr. Stanley Currie, which, by a direct pull, actuate railway signals and points, etc.

2ndly. A complete railway block-system worked and controlled by means of an electric current (generated by a primary or secondary battery), so that complete efficiency and safety are combined with lessened first cost and maintenance.

It is not necessary to describe in detail the work that has been done in electro-magnets up to the invention of the Currie magnet. It will suffice to say that the practical attractive pull of a bobbin on its armature in an ordinary magnet does not extend beyond half an inch, and that the attraction increases inversely as the square of distance for the greater part of the range, and for part of the stroke as the cube. As a consequence, the length of range is not sufficient to get a practical useful pull, and the impact is

destructive. If, however, multiplying gear is used, a proportionate increase of size of magnet and strength of current are necessary, and the damaging effect of final impact is increased in an equal ratio.

On the other hand, if any of the numerous solenoid arrangements are used, though a long and even pull can be got, the weight of the bobbin is excessive, and the final or holding pull is of no practical value.

Currie Long-pull Magnet.

In the long-pull electro-magnet, shown full size in the accompanying diagram, Plate I., Fig. 1, which is the invention of Mr. Stanley Currie, the principle adopted is a combination of the horse-shoe magnet and solenoid, with additions; but the construction is so materially modified as to give far greater power and efficiency, and the magnetic attraction is more evenly distributed over a longer range, while the initial pull is stronger and acts at a greater distance than in any other electro-magnet of the same weight and with the same current. The range already attained in practice is $3\frac{1}{2}''$, and can be increased.

The bobbin, B, here shown standing upright on one end, is made with a tubular core, C, of soft iron, and the coil of wire, KK, wound around it is surrounded by an outer casing of soft iron, B, of the same weight as the core, with a soft iron base-plate at the bottom of the bobbin, connecting the core with the outer casing; a brass plate, V, covers the wire at the top. The copper wire used in the coil is No. 18 B.W.G., or $0{\cdot}048''$ thick. The armature consists of three portions, each one playing its part in the work to be done. The central stalk, A, of soft iron is rather shorter than its own range of motion, and is encased in a brass tube, T, which is prolonged below it so as to form a guide fitting within the bobbin core. The soft iron cap or disc, D, fastened on the top of the central stalk, is slightly larger in diameter than the outer casing of the bobbin, B. It is made by preference of two or more thicknesses of flat plate, to assist in demagnetisation, but it must be thick enough to prevent saturation with any working current. Round the *edge of the* disc runs a cylindrical rim or

flange, F, projecting downwards; it is so shaped as to suit the attraction required, and it comes within the range of attraction of the outer casing of the bobbin when the lower end of the central stalk has entered within the core. When the rim in turn has done its duty, the disc, D, comes within range of attraction of both outer casing and inner core.

Uniformity of Pull.—So long as the central stalk or armature rod is altogether out of the bobbin core, the attraction upon it continues to be inversely as the square of its distance from the bobbin; but as soon as the end of the rod enters within the orifice of the core, the force of attraction becomes lost upon so much of its length as is inside the core. The same diminution in attractive force holds good in regard to the flanged rim of the armature disc as soon as its lower edge passes below the upper edge of the bobbin. The force of attraction varies also directly as the mass of the body attracted. Advantage is therefore taken of these two principles in combination, to regulate or adjust the diagram of pull in such a way as to obtain some sort of approximation towards uniformity throughout the $3\frac{1}{2}''$ range of stroke of the armature. This is accomplished by tapering, in the manner shown in Figure 1, the lower end of the armature stalk, A, and also the flanged rim, F, according to the desired adjustment of the attractive force; in addition to which the thickness of the armature disc, D, and the distance that its flanged rim, F, projects downwards, as well as the thickness of the rim, can also be varied; and in some cases the bottom edge of the flange is made of a serrated or wavy form. The result is that, when the strength of the pull on the armature stalk and flanged rim is decreasing, owing to their having both of them reached and passed the position of maximum attraction, the pull on the disc is increasing as it nears the bobbin.

By suitably adjusting the proportions of the various parts, the pull can be so varied in force and range that it can be adapted to meet almost any requirements.

It must be noticed here, in connection with the working of signals, that the diagram of resistances of the signal counter-*weight and* the pull of the magnet can be made to follow

practically the same line—in other words, to have the same values.

Double Length of Pull.—A double length of pull is readily obtained by the simple tandem combination shown in Plate II., Fig 3, U L.

Here a pair of single magnets on the foregoing principle, arranged at a fixed distance apart, have their armature guide-rod, R, in common; the lower armature disc, L, is made fast upon the rod, while the upper disk, U, bears against a shoulder upon it. The range of the lower disk being nearly double that of the other, the first part of the pull is given by the upper, and by the time the upper disc, U, has closed upon its own bobbin it has brought the lower disc, L, within the attractive range of the lower magnet, by which the second part of the pull is then given, the armature rod, R, now sliding free through the upper disc, U. This arrangement is suitable for working signal arms that are required to stand at the three positions of "danger," "caution," and "line clear," as shown in Fig 3, Plate II.

Railway Signals.—In the application of the electro-magnets to railway signals, the ordinary signal posts and arms are utilised; but it is advisable that the bearings and working parts should be made as true as possible, because, though friction is not so material when the work is done by manual labour, it is of the utmost importance that it should be reduced to a minimum where electricity is the motive power. As there are no complicated parts, and the movements are all simple and direct, there is no difficulty on this point.

In the accompanying drawing, Plate I., Fig. 2, is shown the application of a single magnet to an ordinary signal arm intended to stand in only the two positions of "danger" and "line clear." The magnet, M, is fixed upright on a bracket at the back of the post, and a chain from its armature pulls upon a disc centered on a horizontal spindle above it. The disc, O, carries a lever and counterweight, W, acting in opposition to the pull of the magnet; and also an arm, R, which is connected by a rod, P, with a bell-crank centered at the side of the post. Another rod, S, connects the bell-crank *with the semaphore* and spectacle. When the

magnet is out of action, the semaphore is held up at "danger" by the weight of the spectacle, in conjunction with the counter-weight, W, on the lever, which then comes against a stop, V. In this position all the parts are locked, in consequence of the arm, R, being then on its dead-centre—that is, the arm and bell-crank are so arranged that the direction of the rod, P, connecting them passes then through the centre on which the disc turns. For yet greater security of locking, it is preferable, indeed, to let the connecting-rod be even a trifle beyond the dead-centre, so that any pull upon the bell-crank, from wind pressure or accumulation of snow on the semaphore, shall hold the counterweight lever, W, still more firmly against its stop. The locking is thus done mechanically, and is independent of the magnet. On bringing the magnet into action by a current from the signal-box, the pull of the armature rotates the disc, raising the counterweight and spectacle, and lowering the semaphore; in this position the semaphore is retained so long as the electric current is continued. On the cessation of the current, the semaphore is automatically raised again to "danger" and locked there mechanically without the aid of extraneous catches, which is a most important feature.

In Fig. 3, Plate II., is shown the application of the double magnet or tandem arrangement to a balanced semaphore centered at mid-length. Here the pair of magnets are fixed on one side of the post and the disc on which they exert their pull is fixed on the spectacle spindle. A rod, R, connects the spectacle with a crank on the semaphore, and the weight of the spectacle brings the semaphore to the horizontal position of "danger," in which it is locked mechanically, as before, by the connecting-rod being then on its dead-centre. An electric current sent to the upper magnet pulls the semaphore to an angle of 45°, "caution;" and a second current sent to the lower magnet pulls it vertical for "line clear." In either position it is held by the spectacle acting as a counter-weight against the retaining pull of the magnet.

Railway Points.—In Fig. 5, Plate II., are shown the magnets and gearing for working a pair of railway points. These magnets *are practically* of the same construction as for working signals,

but are of larger size, and are wound with copper tape instead of wire, in order that they may take a maximum current with a minimum of resistance. With a current of 23 ampères and an electro-motive force of 40 volts, the force of the pull is as shown by the dotted line in Fig. 4, Plate II., commencing with an initial pull of 33 lbs. at 3½″ distance, and increasing to 54 lbs. at 3″, and to a home pull of 1,064 lbs. As will be seen from the diagram, the points are pulled over and held in either position by the sliding-rod, R, which is worked by the lever, and is locked by the locking-bolt. The slot in the rod, R, is made ½″ longer than the width of the lever working in it, so that the first ½″ travel of the lever withdraws the locking-bolt by means of the incline on the extremity of the lever, before the lever acts upon the sliding-rod, R.

Where a pair of points are covered by a signal, the locking-bolt, in conjunction with the armature of the magnet which pulls the points over into the position corresponding with the signal when down for "line clear," completes the circuit which enables the signalman to lower the signal. The signal is checked by its automatic repeater in the signal-box. The other point magnet completes with the locking-bolt the circuit which works a repeater in the signal-box.

Electric Current.—For working signals it is necessary to use a maximum current for lowering them, and a minimum current for holding them down "free," so that their normal position may be at "danger," and also for obvious economical reasons.

From the full line in the diagram Fig. 4, Plate II., it will be seen that with 5 ampères an initial pull of 8 lbs. at a distance of 3½″ is given, and the home pull is 321 lbs. There are various ways of reducing the lowering current to a retaining one. It is preferable, however, to switch out the main battery in the case of a primary battery, but where a secondary battery is used a resistance may be automatically inserted in the circuit. Supposing each signal is lowered 150 times in each 24 hours, and that each lowering takes 2 seconds, and that 10 ampères is the lowering current and ·2 the retaining current, and 5 ohms is the circuit resistance — $150 \times 2 = 300 =$ S.

Then lowering current $C = 10$; $R = 5$, $E = 50$, and $C^2 R = 500$, and H.P. $= \frac{C^2 R}{746} = \cdot 67$.

H.P. s. $= \frac{C^2 R \, . \, S}{746} = 201$ and H.P. h. $= \cdot 05535$ per signal per diem. Then retaining current which may be supposed to be running for 12 hours out of each 24,

$C = \cdot 2$; $R = 5$, $E = 1$, and $C^2 R = \cdot 04$ and H.P. $= \frac{C^2 R}{746}$ and H.P. h. $= \cdot 00321$.

Thus $\cdot 05535 + \cdot 00321 = \cdot 05856$ per signal per diem, allowing 50 per cent. loss of battery in use at 2d. per H.P. per hour, then $\cdot 05856 \times 2 \times 2$d. $= \cdot 232$d., or less than $\frac{1}{4}$d. per diem.

The expenditure of the electric current in connection with the working of points only takes place while the points are being moved, as they are automatically locked in position as already described.

Advantages.—The advantages of working signals and points by this system are that their distance from the signal-box is immaterial, inasmuch as the electric working gets rid of all the mechanical difficulties which arise from excessive expansion and contraction of lever wires and from the severe pull required to work them through long distances. And in case of distant signals and those in tunnels, irrespective of distance and position, the three positions can be used, and a perfect check or tell-tale, R, maintained in the signal-cabin. The advantages in the working of points are that all ground rodding is done away with; the points may be in any convenient position without regard to distance from cabin.

Application of the Whole System.

In Fig. 6, Plate III., is given an illustration of the application of this electric system to working the signals and points of an ordinary junction where there is a double line of way both on the main line and on the branch. The signal-box is here divided as regards the electrical connections, levers, and switches, into thirteen divisions, 1 to 13.

Suppose an up main line train is required to be turned into

the branch, it will be necessary to work the points P 6 and the three signals on the up main line, namely, the distant signal, S 1, the home signal, S 3, and the starting signal, S 5. The lever of the points P 6 is first pulled over the division 6 of the signal box, whereby an electric connection is made between 6 and 7, the up branch points being thus set right for the up train to pass into the branch. The down branch point lever, 8, must be back or in its normal position, in order to let the current pass through to division 3 for lowering the home signal, S 3. As soon as the signal lever is pulled over and the signal lowered, the current can pass on to 1, for enabling the distant signal, S 1, to be lowered. Last of all, the current can pass to 5, lowering the starting signal, S 5.

Suppose another case of a train approaching the junction on the down branch, to pass upon the down main line; the home and distant down branch signals, S 12 and S 13, will have to be lowered. The points P 8 must first be in their normal position, and their lever, 8, must be back, before the electric current from the battery can pass through 8 to 12 and thence to 10, for lowering the home signal, S 12, and afterwards to 13 for lowering the distant signal, S 13. It is now impossible to lower the home and distant signals, S 10 and S 11, of the down main line, because the current for working these signals is prevented from passing by the act of pulling forward the lever that belongs to the home signal, S 12, of the down branch.

Any systems or series of systems can be arranged in a very small space, and so that, if any wrong lever or switch is moved in working a given system, a bell is rung and the signals go to danger.

When a train passes a signal post, it, by the deflection of the rail, breaks the circuit through which the electric current runs to the magnet, and automatically puts the signal to danger.

Plate IV., Figs. 7 and 8.—The signalman in cabin B cannot lower his departure signal till permission is given to him by station C. This is a usual procedure; but the special feature throughout this system is the use of a continuous current always running when *work has to be done*, and this current, whether

working signals or points or block instruments or repeaters, automatically, stopped thus effectually combining safety an economy.

In Plate IV., Fig. 9, the signal switches are shown at 1, 2, 3, 4, 5, 9, 10, 11, 12, 13, and the point levers (here worked by hand and electrically locked with signals) by Nos. 6, 7, 8.

Mr. LANCASTER OWEN: I have been called upon by the President to express my views on the subject of Currie's long-pull magnet as applied to railway signals.

I must preface my remarks by saying that I do not profess to be an electrician, but a railway engineer.

We railway engineers have for a very long time been seeking a more effectual and reliable method of actuating distant signals than the present arrangement of wires, etc., which are, as is well known, affected by temperature and other extraneous influences.

A great deal of pains and cost has been expended in experimenting upon all kinds of mechanical appliances for this purpose, and more especially for modifying the effects of changes of temperature upon the wires.

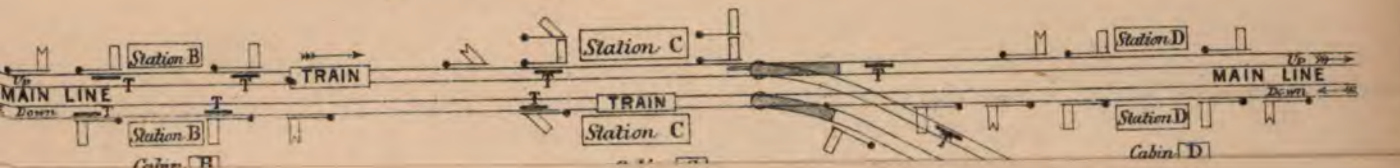
The long-pull magnet invented by Mr. Currie has lately been brought to my notice, and, as far as I can judge, it appears to give us exactly what we have been seeking for.

We shall, I hope, be able to abandon the use of the existing mechanical appliances, the complicated arrangements for adjusting signal wires, and the use of heavy levers, with repeaters and gearing, and, as far as I can judge, we shall be able to pull down the signal arms by means of this long-pull magnet in a very much more effectual manner, and with much more ease and certainty than has hitherto been the case.

As I said before, I do not profess to be an electrician. I leave the electrical part of the business to the gentlemen belonging to this Society, who are experts, and who will be able to discuss it with an authority and knowledge which no one not being a trained and experienced electrical engineer could hope to attain to.

Mr. LANGDON: I do not think I can add much to the interest of the discussion, but I should like to ask Mr. Timmis to state,

Fig. 7. Signalling from Station to Station.



when he is replying to the remarks which may be offered during the discussion, whether he has had any opportunity of practically testing his method of working the points. We know that railway points are frequently placed in very awkward positions. I take it that his magnets could scarcely be placed underground, and therefore they must be placed somewhere above-ground. In such a position they would necessarily have to be protected in some way. They must be covered by some hut, or structure of that kind, and it appears to me that there would be some inconvenience attending the finding of a convenient place for such a hut. If, then, the hut has to be placed at some distance from the points, these will be productive of a great deal of friction. Rodding must be used, but to what extent would be regulated by the circumstances which attended the arrangement of the points to be operated. This seems to me to be a very serious question, and I have no doubt Mr. Timmis has given it due consideration. I quite concur with the gentleman who has last spoken, that the magnet will probably prove of service for distant signals. Distant signals have sometimes to be placed in very awkward positions, and it is important that they should be placed well within view of the engine-drivers. This is not always well obtainable at the present time with the mechanical arrangement, but by the aid of electricity such signals can be worked any distance: it is only a question of battery power. But with regard to the general scheme, I hardly think that I can look upon it as one which is likely to supersede the present mechanical arrangement, with economy. For a large signal-box, it seems to me that a very great deal of attention would be required, and in case of failure that we might almost require to have an electrician on the ground. We know that in all electrical apparatus failures will take place in the same manner as with mechanical apparatus.

An electrical system no doubt affords a more ready means of accomplishing that which is recognised as "interlocking"—that is, you have by it a ready means of disconnecting or connecting up your circuits. There is that advantage in Mr. Timmis's arrangement; but should any failure take place, I am very much of opinion that it *would call for the presence of* an electrician—certainly

somebody possessed of more ability and intelligence than the ordinary lineman. This, as regards the commercial aspect of the question, is my view of the case. I think the arrangement will be of service for distant signals; but in its application to the general management and working of a signal-box I think that it would be well to obtain some practical experience before we venture very far.

Sir CHARLES BRIGHT then illustrated, by drawings on the board, Mr. Edward Dering's and Count de Moleyn's machines, made many years ago with the object of attaining a long pull or stroke with electro-magnets.

Professor HUGHES: I have for many years taken a great interest in the subject of long-pull magnets, and Mr. Currie's resolution of the problem seems well worthy of attention.

We know that an ordinary electro-magnet has a strong but unequal force through a small range.

Page of Washington tried to eliminate these defects, and, after a series of most remarkable experiments, extending from 1837 to 1850, upon different forms of electro-magnets most suitable to be employed as a motor, finally adopted what he termed the axial force, viz., that by means of which an iron core is drawn into a coil. I had the pleasure of visiting Mr. Page in 1855, and he showed me his electro-motors, the most powerful of which consisted of a series of coils superposed.

The electrical connections being so arranged that these coils were brought successively into action, the coils were wound with square insulated copper wire, each coil being about eighteen inches diameter and eight inches deep, with a central opening of

FIG. 1.

seven inches. An iron rod three feet long, six inches diameter, *was drawn into* these coils by the axial force, producing a stroke

or length of pull of two feet. The length of pull, however, could be increased to any desired length by adding more coils.

In appearance it resembled the cylinder and piston of a steam-engine, and, like the piston, the rod was geared to a crank by a crank shaft, as shown in Fig. 1, where A′A″A‴A⁗ are a series of coils, B the iron rod, C the shaft, and D the crank.

Page certainly obtained remarkable results, for when using his coils and rod as an electric hammer, he could by its means raise vertically 500 pounds with a stroke of two feet. He employed his motor in driving a circular saw, which would rapidly saw boards of two inches in thickness. The most remarkable application that he made was the driving of an ordinary large-size American railway carriage, employing for this two of his motors, each being connected directly to the car wheels. The speed obtained was fifteen miles per hour. It failed, however, in practice, owing to the cost and short duration of Grove's battery (the most powerful source of electricity available at that time). If Page had possessed, as we now possess, the comparative economic and powerful currents of our dynamo machines, his electric railway would have been an undoubted success.

Many attempts were made in England, amongst whom Mr. Allan (1854) produced a long-stroke engine, by employing a series of electro-magnets acting successively upon a series of iron discs hung loose upon a central rod, but bearing upon shoulders upon this rod at the desired interval, in a manner somewhat similar to the tandem combination as shown by Mr. Currie.

Froment, in Paris, made numerous experimental forms of long-pull magnets, and adopted for his electric clocks a form by means of which he could obtain an equal mechanical force from the unequal force of attraction at different distances of an electro-magnet upon its armature.

He employed a system of double levers, somewhat similar to the toggle joint of a printing-press.

This is shown in Fig. 2, where A is the electro-magnet, B the armature, C′C″ double levers attached to the armature and fixed pivot, E. The central junction of the levers gives rise to a motion of the *connecting-rod, D,* whose position can be regulated

so as to have an equal force from the variable power of the armature.

In Fig. 3, the electro-magnets, A′A″, act equally upon the armature, B, producing a perpendicular pull or movement of the armature of about two inches.

A very remarkable compensation for the variable force was invented and applied by Robert Houdin. The armature, by

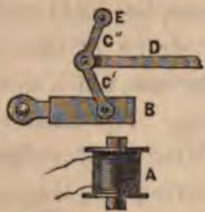


Fig. 2.

Fig. 3.

means of a curved lever acting upon a second lever, changed its leverage in proportion to the increase of force, producing a remarkable equal power upon the second lever throughout its entire course. This is shown in Fig. 4, where A is the electro-magnet, B the armature, carrying on its axle the curved lever C. The second lever, D, having a separate axle, E, produces an equal force upon the connecting-rod, F.

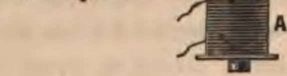


Fig. 4

Whilst engaged upon the invention of my printing telegraph instrument (1848), I found that if I employed the ordinary electro-magnet my instrument would not be a success.

The problem in my case was a difficult one. I required that the armature should move with sufficient power and length of movement to open or unlock a detent connected with the printing *levers. It required* that this movement should be exceedingly

rapid and powerful. And in order that the instrument should be able to work at greater distances and with more feeble currents than possible with the Morse or any instrument requiring sufficient current to produce even a slight motion to an armature, it was required that an exceedingly feeble current should produce a mechanical force of almost unlimited power and rapidity of action.

After numerous hopeless failures, I finally (1849) resolved the problem, in the complete manner which now exists and is employed upon all my instruments throughout Europe.

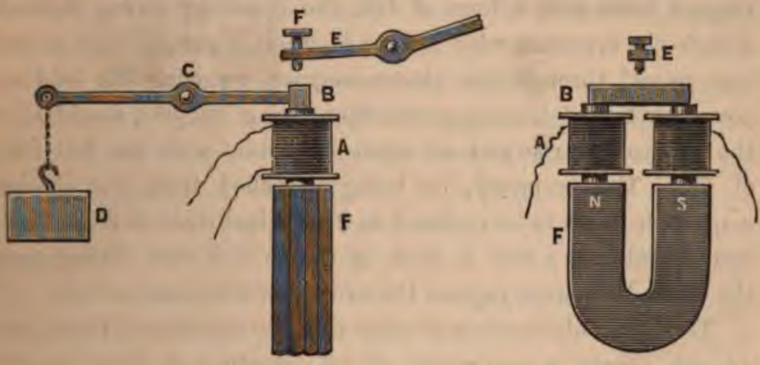


FIG 5. FIG. 6.

A being the electro-magnet polarised by a powerful permanent magnet, F.
B, the armature near or in contact with the poles of the electro-magnet.
C, the armature lever, upon which is applied the balancing or opposing spring or weight, D.
E, the detent or mechanism upon which it is desired that the armature should act with a powerful mechanical force. The adjusting screw, F, of the detent does not touch or rest on the armature, consequently any motion of the armature upon its leaving the electro-magnet is not interfered with by any friction or resistance.

Instead of requiring that a current passing through an electro-magnet should produce a slight motion of an armature, I required that it should produce (as it must in every case) a slight difference in the electro-magnet; and supposing that the armature in contact with the electro-magnet could be so balanced by weights or springs as to be in a state of equlibrium, then the weakest current would destroy this equilibrium, and allow the spring or weight to *act upon the* armature.

The general idea of this system is shown in Fig. 5, the side view, Fig. 6, being the front view.

Let us assume that the permanent magnet polarises the soft iron in cores of the electro-magnet, and that its armature in contact would require 100 pounds at D to detach or balance the holding-power. It is evident that when it became detached from the poles the armature would rise with the full force of the weight, D, and, by a powerful hammer blow, act upon the lever, E.

If we now replace the armature, and balance the holding-power by an adjustable spring, D, so that, whilst the electro-magnet holds with a force of 100, the opposing spring tends to detach the armature with a force of 99, it is evident that a current passed through the electro-magnet, reducing its holding-power by 2 (the holding-power then being but 98), would allow the armature to rise and act upon the detent with the full force of 99. The armature, on being detached from the electro-magnet, requires to be replaced in its original state of repose by a mechanical force: this is done by means of a cam acting upon the lever, E, so as to replace the armature after each action.

Thus the only motions are due solely to mechanical forces, and we can obtain a movement of an armature of great length, rapidity, and force, and this by the action of an extremely feeble current, as we may adjust the differential forces of the holding-power of the magnet and the balancing-spring to any degree of sensitiveness required.

Electro-magnetism in this case acts with its maximum effect, the armature at the time of its action being close or in contact with its poles; whilst in other electro-magnets, such as a sensitive telegraph relay, the armature is at a distance, and the electro-magnetic effect is diminished as the square of this distance.

The practical consequence of the sensitiveness of the magnet I have described is, that my instrument is now working, and has worked for many years, upon the longest direct telegraph lines in Europe, and these are at least double the length that would be worked by the ordinary Morse instrument.

My object in describing this magnet is to show its applicability *to the* working of railway signals. This has been accomplished

with great success by Mr. Lartigue, in France, and the signals of the Northern Railway of France are worked by its means. Mr. Sykes has also applied this magnet in his system of signals applied to the London, Chatham, and Dover Railway.

I do not wish, however, to say that any particular form of electro-magnet is suitable for all purposes, as evidently the problem which Mr. Currie proposes seems most admirably fulfilled by his long-pull electro-magnet.

Mr. Currie's electro-magnet will no doubt find a field of usefuless in many valuable applications; and I can only express my admiration at the simple manner in which he has achieved the success that he has shown us this evening.

Mr. A. J. S. ADAMS: A remark just made by Professor Hughes regarding the failure of a battery appears to me particularly pertinent to the subject under discussion, because, although the suggested application of electricity to railway switch and signal purposes looks well at first sight, its practical application will eventually show that the authors have reckoned without their host. I imagine that *certainty* is an important factor in the carrying on of such work; whereas battery power—especially in the case of batteries having large plate area—is so proverbially uncertain that it can hardly be expected to perform what is here claimed for it, in a safe or satisfactory manner. Nevertheless, battery and accumulator power might be usefully employed as a means for the application, at a distance, of some other more economical and reliable force. It is unfortunate that the intentions of the authors in respect to the form of electrical power were not explained.

The evils attending the usual mechanical arrangements have been described; but I fail to see the necessity for so expensive and intricate a system as this, when railway engineers have everywhere an abundant water supply, constituting motive power ready to hand, and the application of which, by hydraulics, to switch and signal working, even at great distances, would, it seems to me, be a more simple, certain, and preferable method.

Mr. CONRAD W. COOKE: Professor Hughes has referred to the application to railway working, by M. Lartigue, of the Hughes

magnet—that is to say, a permanent horse-shoe magnet carrying on each pole a coil through which an electric current may be transmitted in such a direction as momentarily to destroy or diminish its sustaining power. I should like to point out that this magnet is employed in the interlocking block-system of Mr. Sykes, which is in use throughout the London, Chatham, and Dover Railway. Some time ago I took considerable interest in Mr. Sykes's experiments, and it was exceedingly interesting to observe, in connection with them, what heavy mechanical effects could be brought about at a distance with so small an expenditure of electricity. The heavy lifting is done by the arm of the signalman in pulling over the levers, an armature is thereby lifted up against the poles of the Hughes magnet, there to be held by magnetic attraction until, by the transmission through its coils of a comparatively feeble current of electricity (in a direction whose tendency is to depolarise the magnet), the sustaining power is destroyed, the armature falls away, and the locking apparatus is released. The great advantage of Professor Hughes's magnet in such an application is that its sustaining power remains as long as no current is transmitted through its coils, and is momentarily destroyed the instant a current is sent through them. There is thus no expenditure of electricity, except at the moment the mechanism has to be released.

Mr. Preece: It is always extremely interesting to hear of what has been done in the past; but there is this disadvantage in listening to mere history, that we can only hear of failures. Now, we had a paper read before us on the last occasion, that was a distinct account of a success; and I think Mr. Timmis has been the first who really has shown in practical operation a long-pull magnet, and a long-pull magnet applied to an extremely useful purpose. I had the great pleasure of being shown by Mr. Timmis the working of signals and the working of his system in his office in Great George Street; and it certainly would well repay any electrician who has the interest of his profession at heart, to pay Mr. Timmis a visit, and see there how he has applied electricity to this practical purpose. Again, Mr. Timmis's *system has* been applied on a practical scale at Swansea, and I

hear from the manager of the Swansea Docks that it is there a decided success. Well, I am, as you all are probably aware, an electrician, and I love my profession. I am not a cobbler, but I am a believer in that old proverb that says "every cobbler should stick to his last;" and, as an electrician, although I perfectly agree in much that Mr. Timmis has said, and in justice to the success of Mr. Timmis's long-pull magnet, yet I say, as an electrician, I do not think Mr. Timmis is right in applying his principle to every condition where electricity should be applied. I do not think, much as I like electricity, that electricity should be used where mechanical power can be used to better advantage, and I think that, in carrying out this hobby, Mr. Timmis has carried it a little bit too far. It is all very well to have long-pull magnets, and to pull down signals at a distance where mechanical power cannot be applied; but it is useless to apply electricity to pull down a signal at home when you can do it with a much less expenditure of power by mechanical means. In his paper he has pointed out that, in order to get a pull of 54 pounds at 3 inches, he has to expend a horse-power and a quarter—that is, 23 ampères, with an E.M.F. of 40 volts, or 920 watts. Well, to use a horse-power and a quarter to get a pull that any child can exert, is to my mind a waste of power, and a wrong application of electricity, unless you apply that electricity in conditions where mechanical power is impossible. Now, there are many such cases on railways. I am an old railway man: it is fifteen years since I had anything to do with railway signals. I had a good deal to do with them, and with the assistance of one of my oldest and best friends, Mr. Langdon, whom I am happy to see here to-night, we applied a great many experiments in extending electricity to the application of signals on railways; but we never dreamt in those days of pulling signals down at a distance by electricity. Now, with what Mr. Timmis has shown us here, and with what we have heard, if I were again to go on a railway, I certainly would try to apply electricity to pulling down signals where mechanical power cannot be usefully or economically applied. And here is a point to which I would like to call the attention of railway engineers. There never has been *an invention that has led* so enormously to the saving of

life and to the security of traffic as the introduction of the block-system on railways. In days when traffic was less we thought it quite sufficient to protect a section of about five miles in length by means of electric signals. As the traffic increased, it became necessary to diminish the distance of these sections, and in many instances we had to insert signal-boxes between the two block-sections, in order to reduce the length of the sections, and so enable a greater number of trains to pass through a given line. Well, this has been carried out to a great extent, and now I think I am right in saying that the average length of a section on a well-protected line is something like a mile. But with the increase of traffic there has arisen a necessity to diminish the length of these sections, and it is here where I see an application of this principle of Mr. Timmis; for you can, with power that he has shown us can be erected at a distance, succeed in placing signals intermediate between the signal-boxes, automatically worked by electricity without the necessity for signalmen, for the expense in carrying out the block-system is not the expense of apparatus, is not the expense of signals, it is the expense of the men. It requires three men to be permanently employed to work a block-station, and if you can by any means dispense with the services of these three men you reduce the expenditure, and certainly put in the hands of railway companies a means of extending the block-system at a reduced expenditure. And it is here where I think that Mr. Timmis should devote his attention, instead of trying to do close at home what you certainly can do very much better by means of mechanical appliances, either by the direct pull of a wire, or, as has been suggested by Mr. Adams, by hydraulic power, or, as I saw in America, applied on a very large scale indeed, pneumatic power. At Pittsburg, one of the busiest stations in the whole of the United States railway system,—a very large station indeed,—the innumerable points and railway signals are worked on the locking and interlocking system entirely by pneumatic pressure; and where pneumatic pressure can be used it certainly is more economical than power which requires the employment of a horse-power and a quarter to obtain, at a comparatively short distance away, the pull of a mere child.

Professor AYRTON: Is not this very large amount of power required to carry out Mr. Timmis's excellent idea of working railway signals electrically, simply because Mr. Timmis is endeavouring to do by means of a long-pull magnet what experience has shown cannot be economically done in that way? Professor Hughes has pointed out to us that in the early days, when people wanted to perform any operation by means of electricity, they thought the only way to do it was to imitate the ordinary hand-saw, and so get a long forwards and backwards motion from a long-pull magnet. But we know now that the proper way to perform any operation electrically is to have a small motor running at a considerable speed, and so performing the operation by means of a small current and with a small expenditure of power. Formerly the main idea was to get from a long-pull magnet something like the motion of a long piston on a steam-engine. Now we do not in the least imitate an ordinary steam-engine in an electro-motor—we imitate, perhaps, a rotary high-speed engine, but not an engine with a long piston—so that it appears to me that the plan to adopt with railway signals is to use little motors and not long-pull magnets; and if you put a little motor attached to the signal-post, and make it by its rapid rotation gradually raise the arm, then, instead of using a horse-power, you fulfil your object by using only an excessively small fraction of a horse-power, exerted it may be for a quarter of a minute. In fact, an electro-motor is the best long-pull magnet known, seeing that the same force is, for the same current, always exerted by the fixed electro-magnet on the rotating one, whatever be the position of the latter.

The PRESIDENT called upon the Secretary to read the following letter on the subject from Mr. Sydney Walker, who was unable to be present at the meeting:—

Remarks on "The Working of Railway Signals and Points by Electro-Magnets, and controlling them, with complete Block System, efficiently and economically by a Current from a Primary or Secondary Battery," by Illius A. Timmis. By SYDNEY F. WALKER, M.I.M.E.

As the author of this paper appeared to consider some remarks that I made *upon his paper on the* same subject, which was

read before the Institute of Mechanical Engineers in October last, as unduly severe, and as he further maintained that in many points I was in error, I challenged him to bring the subject before this Society.

I am not aware if I can claim the honour of being the means thereby of bringing the subject under the notice of the members; but having acted in this way, I am naturally anxious to be able to state my case.

I think that we must all admire the careful and complete manner in which the whole of the arrangements have been worked out; and we must all feel grateful to the author of the paper for having, at considerable trouble and expense, introduced a new, and we may hope a wide field for our labours.

At the same time, I presume we may, without detracting from the merit of the invention as a whole, be allowed to suggest improvements in those parts which form our special study.

It certainly appears to me that Mr. Timmis has not in his paper given sufficiently good reasons for the invention of an entirely new form of electro-magnet.

In my opinion, for anything that is stated in the paper to the contrary, either of the existing forms of electro-magnet would have answered the purpose equally as well, and at a much smaller cost both for installation and maintenance.

Mr. Timmis commences by saying that the old horse-shoe or three-piece magnet will not do, because with it you cannot obtain attraction of an armature at more than $\frac{1}{2}''$ from the poles, whereas he requires a pull of $3\frac{1}{2}''$, and that though you may get your requisite pull of $3\frac{1}{2}''$ by means of some form of multiplying gear, the cost of the current you would be obliged to use would be prohibitive, and in addition the impact of the keeper would be too great for practical work.

In reply to this, I maintain that Mr. Timmis is mistaken in supposing that he could not get more than $\frac{1}{2}''$ pull with the ordinary form of magnet, and that he can get as long a pull as he requires, provided his magnet is large enough, and the poles and armature be so arranged relatively to each other that the lines of *force emanating* from the poles pass through the armature at

the required distance; and I think he has been led to the supposition from the fact that, by the law of the squares, the advantage of having the keeper as near as possible to the poles is so great that no one would construct an apparatus, involving an electro-magnet and its keeper, otherwise, except for a special purpose.

With regard to the question of multiplying gear, I would point out that, by the law of the squares, the same electro-magnet which gives him a pull of 8 lbs. at $3\frac{1}{2}''$ should give him 392 lbs. at $\frac{1}{2}''$; and that a pull of 56 lbs., acting through $\frac{1}{2}''$ on the short end of a lever geared 7 to 1, would give him the required pull of 8 lbs. at $3\frac{1}{2}''$; so that, other things being equal, an electro-magnetic arrangement of one-eighth the power, if arranged in the way I have suggested, should do the work.

Then as to the impact. It does not appear to me that the impact with the multiplying gear should be much greater than that which Mr. Timmis has with the magnet he uses, though of course it may act more gradually.

At the same time, I should say that there ought to be no difficulty in making the apparatus strong enough to withstand the blow.

It appears to me to be an odd thing for a skilled mechanical engineer to say he is unable to provide for the comparatively small impacts that he has to deal with in this case. Has he not far heavier to deal with every day in other forms of apparatus?

Next the author condemns the solenoid magnet, because, though it will give a long stroke, all its attractions are neutralised when in the central position.

I take this to mean that the core would be allowed to recede from the coil when it attained a central position.

This also appears to me not to be correct. I can hardly, in fact, understand the reasoning which has led up to it. The position taken up by the core will be in obedience to the resultant of the forces acting on it: on one side the pulling effect of the current circulating in the coils of wire, and on the other its own weight and the forces acting with it, supposing that the weight acts against the *pull of the coils;* and, whatever position it takes

up in obedience to the resultant force, it appears to me it must retain until one or the other of the conditions undergoes some change.

Solenoid magnets are used in several forms of arc lamp, notably the "Brush," but I certainly have never met with the action Mr. Timmis mentions, and it appears to me that such action would complicate matters very seriously.

The solenoid form would not, undoubtedly, be so economical as the ordinary, but it would be a question whether the convenience of the long stroke would pay for the wasted power.

The actual form of the magnet described, which it is claimed will do what no other magnet will do, appears to me to be a combination of the Altandæ magnet—whose properties were investigated by our late Past-President, Mr. C. V. Walker—and the solenoid, with the addition of an iron tube, stationary, forming part of the core.

Now the one point that was established in relation to the Altandæ magnet was, that it was emphatically a *very* short-pull magnet, though it held on tight enough when it did get hold of the armature; inconveniently so, I found in some experiments I made with it later on.

Therefore I presume that the holding down with a weak current would be the reason for its adoption here; but in taking advantage of that arrangement, and thereby concentrating the lines of force, as that arrangement does, immediately in the neighbourhood of the poles, it appears to me that the pull at a distance—the great feature, it is stated, of the magnet—the great object to be attained to work the signal—is sacrificed, since the lines of force cannot be concentrated near the poles and act at a distance as well. And this seems to be proved by the fact that, taking the author's own figures, he receives a return of 8 lbs. pull at the $3\frac{1}{2}''$ for an expenditure of 91 foot-lbs. per second.

$$W = C^2 R = 5^2 \times 5 = 125 \text{ watts} = \text{about } 91 \text{ foot-lbs. per sec.}$$

On the other hand, the effective pull of the solenoid portion requi[illegible]ed by the strong attraction between it and the iron tube armatu[illegible]h the iron stalk of the armature enters.

force eman[illegible]and, from Mr. Timmis's reply to my remarks at

Nottingham, that he claims this as an important feature in the arrangement.

If by this is meant that the pull is graduated by the brake action of the two cores, and that it is convenient to have the pull graduated in that way, and that the convenience pays for the sacrifice, then the arrangement is intelligible,—"the proof of the pudding is in the eating,"—but it should hardly be labelled economy.

I would point out that the power *provided* for pulling over a point equals $1\frac{1}{4}$ horse-power electrical, which is certainly excessive. Another point on which I differ from the author of the paper is in the strength of the currents used. In my opinion they are too powerful; for the double reason that they are sure to give trouble at the contacts, and that the batteries required to furnish them, if primary, would have to be either excessively large, or require very frequent attention. Large currents, as every telegraph-engineer knows, are always troublesome in a battery.

With secondary batteries, of course, the evil would not be so great; but it must be remembered that even they would require charging less frequently if small currents were used, provided of course they be sufficiently perfect to retain their charge.

I note in a recent communication to the *English Mechanic*, by a gentleman who states that he worked up the Lalande-Chaperon battery, that forty square inches surface are required for each ampère.

I understood Mr. Timmis to say at Nottingham, that you could not do the work with smaller currents; but I think he can hardly be serious in this, and I would point to the series and shunt-wound dynamos where exactly the same magnetic effect is produced by currents varying very considerably in strength.

Further, I would point to the fact, that for economical transmission of power by means of electric currents, the latter must be made as small as possible. The effect produced on the efficiency of a system of transmission by rewinding the machines with finer wire is very striking, as probably many members present will bear witness.

In a conversation that I had the pleasure of holding with Mr.

Stanley Currie, the inventor of the magnet, after the Nottingham meeting, I understood that gentleman to say that he was obliged to use large currents in order to have a reserve, and I believe I understood him (I trust I am not misrepresenting him) that he wished to use even *larger* currents, because the effect rose with the *square of the current.* If I am correct in my supposition as to Mr. Currie's views, he has evidently mistaken the heat generated, or the total work done, for the magnetic effect. Evidently the latter depends simply on the number of turns × by the current strength, while the former depends on the square of the current × by resistance of the wire, so that with a given resistance, or what amounts to the same thing, a given number of turns, the *waste* would be simply proportional to the current strength, or the stronger the current the greater the loss.

I also understood Mr. Currie to say that he objected to increase the resistance of the wire on the electro-magnet, because he ought to have it equal to the internal resistance of the battery. Evidently he was here mistaking the law of maximum *effect* for the law of maximum *efficiency.*

To secure efficiency, it is clear that the resistance of the generator, of whatever form it may be, and the current passing through it, should be as small as possible, since the work done in the generator is not useful work. I think there cannot be any doubt that the work of pulling over signals and points by means of electro-magnets may be done at a very small cost; and, as Mr. Timmis has rightly pointed out, secondary batteries could easily be charged at all the large stations where engine-power is available.

I think, however, that although the cost of battery materials used might be greater, he would gain in simplicity and in the cost of attendance by using smaller currents to pull his signals and points over, as well as to hold them over, and *not* to attempt any switching, whether automatic or otherwise. Every switch, especially every automatic switch, must be a possible source of failure, and the number, therefore, should be as small as possible consistent with the actual requirements of working.

In conclusion, I should like to call attention to one point in connection with the switching from large to small currents. Mr. Timmis states that an incandescent lamp may be used as the resistance which is thrown in to reduce the current strength, and that the lamp will *glow*, thereby serving as an indicator that the current is passing.

In the apparatus he showed at Nottingham the lamp did glow. Now, as the holding-down current is stated to be one-tenth ampère, I think there must be some mistake.

If Mr. Timmis can show us an incandescent lamp which will glow so as to give out an appreciable amount of light with a current of one-tenth ampère, I am sure we shall all be glad to hear of it.

I trust that my remarks will not be taken by Mr. Timmis as in any way wanting in courtesy or consideration for him, or as throwing cold water on the introduction of his apparatus. I am deeply sensible of the advantages to be derived from its use; and in the remarks which I considered it my duty to make at Nottingham, and that I have repeated in substance here, I have merely endeavoured to clear away what appeared to me to be some erroneous impressions on the author's part, and possibly to assist him in the direction of simplifying his apparatus.

Mr. A. TROTTER: A question has been raised as to the amount of energy expended. I note that on one occasion that 1½ horse-power was mentioned, on another occasion 91 foot-pounds. I presume the whole action of putting a switch over is accomplished in about a second, and that not many foot-pounds are expended in a second; and surely when a switch has to be pulled over, or a semaphore has to be pulled down,—whether by a magnet or spinning motor,—the same amount of *energy in foot-pounds* would be expended in both cases.

Professor AYRTON: In order to produce a horse-power for a very short time, you must have a very large battery indeed, even if you want to produce the power for only a tenth of a second. To pull up the signal-arm by one pull, from even Mr. Timmis's long-range magnet, requires the exercise of a large horse-power—it is true, for a very short time, but you must have

a large number of cells to produce this large power at all. It is true that the battery would be quite idle for the rest of the time, but I do not know that that would be a special recommendation to the system.

The other question is, Would a motor require as much power? No; because the motor takes a much longer time to perform the operation: it may take several seconds, or even half a minute. To raise the semaphore requires a definite amount of work,—an expenditure of a definite number of foot-pounds,—but the power necessary to be exerted to do this work depends on the time in which this work is completed; therefore if we use a motor, that is, a long-pull magnet, that can produce a perfectly uniform pull through a distance, so to say, of yards instead of merely inches, and if the exercise of this power is spread over many seconds, and not merely exercised for only one or two, then the power exerted may be comparatively small, and producible therefore by a small battery.

Mr. Beeman: Would the author kindly tell us what is the cost of applying the system to such a junction as that shown by the diagram? Also the cost of maintenance per lever per day on his system? I think these two points are really the essence of the whole affair.

I am informed by railway engineers that they would be prepared to pay almost any amount if a perfectly safe system could be got for working points at least three hundred yards away from the signal-box.

Mr. Currie: As I am responsible for the electrical part of this system, including the invention of the magnet, I think a few remarks from me, in answer to the various questions which have been brought up on electrical points, may be acceptable. I will begin with the statement of Mr. Sydney Walker, in which he says that magnets can be made of the ordinary type to give a longer pull than half an inch. Well, I presume we are all perfectly well aware of this fact, provided they are made large enough. But what I contend is, that since the pull of an ordinary magnet on its armature varies inversely as the square of the distance of the *one from* the other, it thereby exerts a force which is very difficult

(I may say almost impossible) to take full advantage of. If the increasing load on the armature was such that you could make that load vary throughout the stroke directly as the pull of the magnet, you could then make full use of all the forces exerted throughout the whole stroke. The fact, however, of this being impossible, without very complicated mechanical manipulating gear, makes the pull of an ordinary magnet exceedingly wasteful. Even a load increasing as the cosine of angle of inclination of a weighted lever, as shown in drawing, when worked with an ordinary magnet, leaves a large amount of waste power. If, however, the variation of the pull of a magnet can be made to correspond with that of the load on the armature, then not only is the movement even and uniform, but the full power of the magnet is made use of. It is very easy to calculate from the diagrams the total number of foot-pounds per second any given magnet is capable of exerting. I have found one interesting fact in connection with these diagrams, namely, that with a given bobbin and given current, but with a variety in the shapes of the outer rim and centre rod of the armature (the weight being a constant), the area enclosed between the curve of the pull and the vertical and horizontal axes is a constant, or, in other words, the work which the magnet *could* do is in all cases the same. Now, by manipulation of the form of the armature, I can produce almost any required diagram, the forces which are taken off from one part of the stroke being put on, as it were, at another part, thus equalising and economising the work. In the particular case before you, a fair amount of force is left in the last $\frac{1}{4}''$ of the stroke, in order to allow for a small holding or retaining current, which is one of the most important and essential features of the system. Now I have carried out a great number of experiments in order to ascertain the smallest current which is necessary to hold a signal down under any circumstances. I have made these tests during severe gales of wind, and one particularly in the early part of last year, and I have found ·07 ampère with the magnets we use for full-sized signals sufficient. I am thus amply justified in saying $\frac{1}{10}$ of an ampère is sufficient, but we allow for $\frac{1}{5}$, thus giving a good factor of safety. *Mr. Walker* asks me how a lamp can glow

with $\frac{1}{10}$ ampère. By reference to the paper he refers to he will find it is $\frac{1}{5}$ of an ampère, although, as I say, $\frac{1}{10}$ is sufficient. The lamps we use at Gloucester are about 180 to 200 ohms resistance. The electro-motive force of the battery is 40 volts; the resistance of remainder of circuit 10 ohms, including a variable resistance, thus giving ·2105 ampère as the maximum we could have in ordinary use. The lamps *do* glow, but not such as are used for lighting purposes. They are placed in small boxes with black background: the filament being just incandescent gives a most perfect indication of the signals to which the current is running. I hope no one here will make the mistake Mr. Walker apparently does, and try to compare electric signaling on railways by this system (more especially as regards cost) with that of electric lighting. Where the former is in decimals, the latter is in tens. Mr. Walker and others make a great deal of the fact that a point magnet takes $1\frac{1}{4}$ horse-power. Theoretically it does, certainly, but in this case the magnet is purposely run to a high state of saturation and a consequently lower comparative efficiency; for we find from actual practice that, for the sake of space, weight, cost, and the very short time the current is required to run, it is much more economical to do so.

Mr. Walker suggests trouble at the contacts, from the breaking of so large currents. In the most extreme cases we cannot have more than 100 breakings of currents per diem, or 36,500 in the year. Those gentlemen who use dynamos, running at even so low a speed as 500 revolutions per minute, kindly consider the number of breaks in the hour. In fifteen minutes there are more breaks than one of our switches has in a whole year. As a matter of fact, we find no trouble whatever with the contacts. It is also suggested by Mr. Walker that our batteries must be very large. Now it is for the special reason of reducing the size of the batteries that I use what he chooses to term large currents. Theoretically and practically, there is a great saving in space and cost by this means. Had we to transmit great power to considerable distances (say, ten miles), the cost of low-resistance leads would of course be prohibitory; but where the average distance is 600 yards the economy in low resistances is very great. I have

been greatly amused at the terror of those who have been accustomed to use small currents of $\frac{1}{10}$ to $\frac{1}{50}$ ampère, at the idea of six ampères. These, like the remainder of Mr. Walker's suggestions, for which I am greatly obliged, are, however, very far from original. I will briefly summarise them, therefore, by observing that they one and all come within the category of our earlier experiments, which were abandoned more than two years ago. Now, as regards Mr. Walker's elaborate calculations of the size of a Lalande battery, upon data obtained from some newspaper cuttings, viz., one ampère requires forty square inches of zinc, I happen to have myself been specially engaged in experimenting upon this very battery, and have much pleasure in relieving Mr Walker of apparently very erroneous ideas as regards this and any other primary battery, by stating that the resources of a battery for intermittent work are very different to those for continuous work, such as electric lighting, to which the paragraph he refers to applies. I can show Mr. Walker a battery of this type, giving *one ampère per four square inches of zinc surface.* Also one for continuous work, at one ampère per twenty square inches. I should advise Mr. Walker obtaining his data from more authenticated sources. Professor Ayrton suggests that a motor could do the same work as a magnet, with a comparatively small current and small power. This no doubt is the case, as I have myself tried one for this purpose; but the great objection I have to this method is, the small power of the motor having to be transferred by multiplying gear to a larger power, that where a small power is required to work the signal an equally small power could stop it. Thus a piece of dust or grit getting into the motor might hold a signal in position of "free." It is essential that there should be a *clear* force of 40 lbs. tending to start the signal from its "free" position. This cannot be done economically and safely with a motor.

Mr. Preece says the pull of a point switch is no more than a child can do. Now the least pull engineers will allow in a switch is one giving a total of 60 pounds through 4 inches, either direct or manipulated. I do not know whether Mr. Preece has pulled over signal points himself. I presume he has. I myself have

been in a signal-box for many hours in a day, during a period of four months, in order to become thoroughly acquainted with all the necessary requirements. I know that in many cases both point and signal levers require two men to pull them over. Even then it requires a particular knack, and I defy any child of Mr. Preece's or any one else to do it.

Mr. Timmis: I will be very brief in my remarks. I would like to preface them with just one observation, and that is that I have been engaged in the railway world now for a number of years, and it has been my fortune or misfortune to be pretty constantly tugging ahead at some new invention or another, and it is encouraging that I get such a very cordial and, I may say, warm reception as I think I have done this evening. The remarks that have fallen from two or three eminent members of this Society have been to me exceedingly pleasing.

Well, now, Sir, just to take very briefly the different remarks that have fallen, I think that I may first of all notice the remark that fell from Mr. Lancaster Owen, that it is necessary that distant signals should be actuated by a more perfect means than they are at present; and I think I am quoting correctly the remarks of two or three of the principal scientific members of the Board of Trade when I say that it will become necessary by-and-by,—in fact it is necessary now,—if it can be done, that distant signals should be put at a greater distance than they are at present. And it may be—this is, of course, open to exception —that it would be advisable that distant signals should not only be placed at a greater distance than at present, but that they should be worked in three positions, as is shown in Fig. 3, where the double magnet pulls them down either to the angle of 45° ("caution") or vertically ("go ahead"). We know perfectly well that it is impossible, with mechanical appliances, to pull a distant signal down to three positions. It is as much as we can do to pull it down to two. Mr. Langdon alluded to the question of working points with magnets, and he asked for some explanation with regard to the position in which those magnets should be placed. The two magnets are placed exactly between the *points* that have to be moved, and being placed on the sleepers,

can, as a matter of course, be placed at any height that you choose—that is to say, the top of the magnet, over all, can be considerably below, or level with the top of the sleeper. Further, a magnet is placed on a bed which is secured to two sleepers: of course that bed can be so arranged that the points can be sunk below the sleepers themselves.

Mr. LANGDON: My object in asking the question was with reference to wet.

Mr. TIMMIS: I come to that now. Well, then, whatever the height, you can put a wrought-iron plate over the top of the two magnets. There is no difficulty in keeping snow from them, and no difficulty at all about the question of wet, because, as a matter of fact, there is no reason why the bottom should ever get into a wet part, as the magnets can be put outside the rails, and in a vertical position at any height.

I will now take the question of economy of working the magnet. This has been alluded to by several members. It *will* economise time. When the magnet has pulled over the points, a locking-bar is moved, and that is the end of its work. I do not myself, nor does any one, know what an electrical horse-power is; but we will assume $1\frac{1}{4}$ electrical horse-power is required. Well, if it is only used for a part of a second, or, we will say, the whole of a second,—it certainly will not be longer than that,—the economy is easily demonstrated.

I must regretfully pass by the very interesting remarks of Professor Hughes and Sir Charles Bright, on different kinds of electro-magnets which have been invented before. I may say that there are few long-pull magnets that have been invented that I have not gone into very thoroughly with Mr. Currie; but, of course, they say every woman thinks her own baby the best, and I am inclined to think that our long-pull magnet is the best.

Mr. Adams asked what necessity there was for so much expense. I would very briefly answer him in this way. In the first place, with regard to the suggestion he made to use hydraulic power, hydraulic power has been used over and over again. But, unfortunately, the water freezes; and to prevent its freezing by the addition of alcohol is *impracticable*. I may say, in supplementing

that observation, that I was talking some months ago with a member of the railway world in America, and his report to me on the hydraulic systems they had been using there was that they were "an utter failure." I need not go into the question of the reasons why Mr. Cooke made a remark about a mechanical effect and a magnetic one, and said that if the mechanical effect were introduced, and then a magnetic effect to control it, that it would be a much more economical system than this system. That has been worked in America for a long time,—I may say for a couple of years, on the Central Hudson,—but the intricacy of the system, if you get anything like the perfection we want and must have in this country, is far greater than this system of ours, and, in addition, it is far more liable to get out of order and to stick. Interlocking *per se* is a thing which I set my face against entirely. If you interlock, and your signals go to "danger," they must be locked to "danger;" but the question is, how are you going to get that lock out before the signal can be freed, or *vice versa?* In the system Mr. Currie and myself have worked out, we make a very great point of this: that the long-pull magnet, in the first instance, pulls the signal down with a maximum current, then holds the signal down with a minimum current, and if anything goes wrong—not that we expect that anything will go wrong—the signal goes to "danger" (that is the normal position of all signals), and it is locked there without any catch.

When we have done all this work, comes the question that the last speaker asked, What is going to be the cost of it all? "That is the essence," says he, and very properly so. I have worked out the whole thing with Mr. Currie very carefully. At Swansea the system has been working now for seven months, with 150 trains passing the signal every day, and, as Mr. Pearce very kindly and generously offered the remark just now, the superintendent of the Swansea Dock and Harbour, Mr. Capper, has said there has never been a single failure or sensation of failure during the whole of that time. The absolute cost per signal is less than a farthing during the twenty-four hours; and I am prepared to demonstrate to any member of this Society that no signal worked on this system *would cost more* than one farthing per signal per day electrically.

Well, now, if I may be allowed just for one moment, I should like to pass on to the remarks of Mr. Preece. He has referred to the expenditure of horse-power in working our point system. Of course, 54 pounds at 3″, taking $1\frac{1}{4}$ horse-power, is, as he says, rather a difficult thing to swallow; but then, as I have already pointed out, it is for such a very short time that it does not do anybody any harm, because it does not practically cost anything. Now, the block-system,—this is the concluding observation I have to make,—Mr. Preece says, has done more to save life than anything which has been introduced into the railway world. Well, now, Sir, anybody who can do anything to perfect that system, to make it more simple and reliable,—I am leaving alone now the question of economy as regards pounds, shillings, and pence,—is doing a duty which it is necessary for everybody that can to help him. There is no question about it that the great bugbear of the Board of Trade at the present time is, and has been for a long time, the fact of the enormous loss of life amongst the railway servants. The number of men who are killed in railway work between Christmas and Christmas would hardly be believed if I were to go into the statistics at the present time; and I am sure if we can make this block-system more easily worked, and more reliable, we shall be doing, at all events, what is worthy of the attention of the railway and electrical world. If you are going in for any complete system you cannot help yourself; you must have electricity, and you will have to have a system of direct working and interlocking such as I have put forward. As regards the question of points, at the present time the Board of Trade say you must not go beyond 150 yards. Why? Because we know that mechanically these points have failed over and over again. Well, now, I think any one who knows anything about railway work will be aware of the enormous increase in sidings that has to be made in all great junctions and large central depôts. These have all to converge to convenient points for the signal-cabins, and if you took them to suitable distances, you would have to multiply your boxes and cabins all over the place. Now, with our system (take the case of Peterborough) you can *put up one small signal*-cabin, with a post-office girl to

work it, and she could manage the whole of these sidings, and two or three times the number, without any difficulty at all.

Now, Sir, I have attempted to demonstrate that as regards the cost we are economical, and that we are able to work our points any distance economically; that the signals are worked on a correct system, because their normal position is at "danger;" and that the whole system is—at all events, I think so—perfect and complete.

The PRESIDENT: As it is so late I shall not detain you with any remarks from myself, much as I should have liked to join in a discussion of such personal interest; but I will ask you simply to accord to Mr. Timmis a vote of thanks for his excellent paper.

A ballot then took place, at which the following were elected, and the meeting was adjourned until 12th March:—

Members:

Thomas Parker.	James Swinburne.

Associates:

F. H. Badger.	Lewes B. McFarland.
W. Lant Carpenter, B.A., B.Sc.	George Arbuthnot Moore.
William James Dickson.	James Robert Pickering.
Walter Bede Greener.	Evelyn Edward Porter.
J. G. Holdsworth.	Donald R. Ross.
Captain W. R. Lugar.	Richard Mousley Somers.

Student:

Arthur William Slater.

The One Hundred and Forty-second Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 12th March, 1885—C. E. SPAGNOLETTI, Esq., M. Inst. C.E., in the Chair.

The minutes of the previous meeting were read and confirmed.

The following transfer from the class of Students to that of Associates was announced:—

Samuel Joyce, Jun.

Donations to the Library were announced as having been received since the last meeting from the following:—Willoughby Smith (Past-President), Sydney F. Walker (Member), the Director-General of Telegraphs in India, Latimer Clark (Past-President); to all of whom a vote of thanks was accorded.

The PRESIDENT then called upon Sir David Salomons to read his paper.

ON CONSTANT ELECTRO-MOTIVE FORCE IN AN ELECTRIC LIGHT CIRCUIT.

By Sir DAVID SALOMONS, Bart., Member.

It is proposed, first, to investigate a few points concerning dynamos used with accumulators for the purpose of regulating the lamps in incandescent installations, and therefrom find some new deductions.

So far, it would appear that little attention has been given to this subject, and, in consequence, this paper has been written with the desire of helping those who may require assistance, and to propose a new starting-point in the construction of dynamos.

Although a considerable proportion of this paper is theoretical, yet abundant experiments were made before coming to any conclusions, and it is hoped that the result is not entirely without a successful issue.

Let the following points be assumed:—

(*a*) That the resistance of the leads be considered as *nil*, unless mentioned otherwise.

(*b*) That in all cases we are dealing with an incandescent light installation.

(*c*) That in all cases simple shunt dynamos are used.

(*d*) That the engines and dynamos employed are of ample size.

(*e*) That the accumulator is connected up in "parallel."

Now, for an increased resistance r in any circuit, that the current ($=$ C) shall remain unchanged, an increased E.M.F. ($= e$) must be added, and if the original E.M.F. $=$ E, and the original resistance $=$ R, then

$$\frac{e}{r} = \frac{\mathrm{E}}{\mathrm{R}} \text{ and } \frac{\mathrm{E}}{\mathrm{R}} = \mathrm{C} \therefore e = \mathrm{C}\, r.$$

Hence, the E.M.F. must always increase by a numerical value which equals the added resistance in ohms multiplied by the original current in ampères. Further,

$$r = \frac{e}{\mathrm{C}} = \mathrm{C}^{-1} e,$$

that is, if the E.M.F. vary, then, to keep C constant, the added resistance must equal $\mathrm{C}^{-1} e$ (numerical value) ohms.

The reasons why the lamps in any installation are not usually steady are—firstly, that the motive power is irregular, or, in other words, the E.M.F. varies; secondly, the resistance of the circuit may vary and the dynamo not be capable of giving the required E.M.F. for all values of the current taken off; thirdly, a combination of both may exist.

It is seen that if the relations $e = \mathrm{C}\, r$ and $r = \mathrm{C}^{-1} e$ can be made to exist, the light will be perfectly steady.

In practice there is no convenient way of carrying out directly these relations, but the following are five methods of keeping the E.M.F. at the lamps constant, or nearly so:—

(1) By a regular motor.

(2) By the use of accumulators working alone.

(3) By the use of accumulators placed parallel in the circuit whilst the dynamo is running.

(4) By placing in parallel any counter E.M.F.

(5) By various automatic means, magnetic and otherwise.

Method (1) can only be obtained under certain conditions

which will not here be dealt with; method (2) is practically perfect; (3) is approximate, and will be the one now to be investigated; (4) is a new method, to be described; (5) will not be considered in this paper.

Installations at 100 volts are in very general use, in which cases 50 cells of an accumulator are required at a time (if E.P.S. cells are used), each cell giving about two volts, and the resistance of all the cells equal approximately one-tenth of an ohm.

In general it is found that the best charging current, not to injure the cells, is that which has an E.M.F. ten per cent. higher than that of cells to be charged.* Hence, with an installation as above mentioned, 10 volts will be the margin, and we get $C = \frac{10}{0 \cdot 1} = 100$ ampères.

Now, in point of fact, with most known cells under such conditions about 30 ampères pass. Hence, taking the resistance of the leads,† connections, etc., altogether, and probably some internal chemical actions besides, the resistance works out about 0·3 of an ohm. Let this be so.

Let the adjustments of the installation be so made that the mean E.M.F. equal the correct working E.M.F. required for the lamps.

Then any rise or fall of E.M.F. due to the want of regularity in the motor must be neutralised, or nearly so, in order to make no perceptible difference in the light given by the lamps. One point is certain—that if, under the conditions mentioned, the E.M.F. should rise 1 per cent., the increased current sent through the cells will be 3 ampères, and so on in proportion for any other rise.

Now, in simple shunt machines whose brushes require much lead, the E.M.F. rises with a decreased flow of current in the outer circuit. Therefore a fall in the speed of the dynamo may be

* The required difference of E.M.F. to obtain the best charging current varies with the cells employed.

† Messrs. Siemens's method of describing their conductors is the number of yards to 0·1 ohm. It is safe to pass the same number of ampères as the number of the conductor for short periods, and half its number for continuous working.

made to raise the E.M.F., and, on the other hand, the E.M.F. may be lowered in the case of a rise in the speed.

The important part played by the accumulator is due to the fact that its resistance is small as compared with that of the lamp circuit, and this is essential, for at those *moments* when the speed rises, and the E.M.F. rises with it, and the flow of current in consequence increases, all, or almost the whole, of this increase passes through the cells, so that no alteration takes place in the current flowing through the lamp circuit; hence the E.M.F. remains practically unaltered at the lamps for any rise of E.M.F. at the terminals of the dynamo. Therefore, if a dynamo, as described and suitably constructed, is employed, very perfect regulation is obtained by the use of cells, whether the E.M.F. rises above or falls below the normal; the cells and dynamo combined neutralising the rise of, and the dynamo making good the fall of E.M.F., since the variations in the speed are momentary.

If the increase of speed were to continue, there would be a general rise of E.M.F. throughout the circuit—that is, rapid variations would be counterbalanced, but a permanent and considerable rise in the speed would cause a slight and permanent rise of E.M.F. throughout the circuit.

From the foregoing we may conclude the following facts:—

(1) That it is necessary to use dynamos requiring a considerable lead for the brushes, so that the E.M.F. shall rise as the current flowing in the outside circuit decreases, and *vice versâ*, if it is required to steady the light given by the lamps by means of accumulators.

(2) That simple shunt machines, requiring practically no lead for the brushes, and consequently an E.M.F. almost constant for all values of the current flowing in the outer circuit (especially if the magnets are nearly saturated), are not so suitable for use with accumulators for the purpose of steadying the light, although they can be used with good results for charging the cells. For example, a Siemens' SSD_{10} machine (for 300 lamps of 20 candle-power), whose brushes require a considerable lead: the variable current to *the cells due to* variations in the speed of the engine was 2 ampères

(25 to 27 ampères to the cells). At the same time, the variation in the lamp-circuit due to any rise or fall of E.M.F. was absolutely *nil*, being steady at 24 ampères. The experiment was then repeated under the same conditions with a Crompton 14-unit dynamo (for 250 lamps of 20 candle-power), requiring a small lead for the brushes. Result: variable current to cells 2 to 3 ampères (25 to 28 ampères); variable current in lamp-circuit 1 ampère (24 to 25 ampères). Consequently this dynamo did not regulate quite so well.*

(3) That, in any particular installation, any apparatus or machine giving a counter E.M.F. equal to that of the accumulator in use, and having a like resistance, will steady the light just as well as the cells. This method the author believes to be quite new, and one capable of considerable development. For instance, a motor working parallel in the circuit, giving a constant counter E.M.F. (a motor not difficult to arrange), and having an armature of very low resistance, will effectually steady the lights with a very small expenditure of power. The chief advantage of cells is that most of the power is stored chemically, whereas in the motor-regulator it is lost; yet for many purposes the end gained is worth this loss many times over. It must also be observed that any motor, in a factory or elsewhere, with an unvarying load, and using a large current as compared with the "variable regulating current," will become a good motor-regulator, with practically no loss of power. In most cases where motors are in use in parallel with lamps, some method can always be devised for making the motor regulate at the same time.

For another method, suppose the counter E.M.F. of the motor-regulator to vary, it will evidently do so in proportion to the variations of the E.M.F. in the lamp-circuit. Hence the motor might regulate the steam flowing to the steam-engine, or gas going to a gas-engine, in such a way as to regulate the speed of the engine; hence the E.M.F. very like an electrical governor in its action; or the variations in the motor may vary the speed of the

* This illustration must not be taken as expressing any opinion on the relative value of the dynamos, but only to indicate the action of each type when employed with an accumulator.

dynamo, leaving the speed of the engine unaltered. Or variable resistances might be inserted in the main leads, or in the F.M. shunt-coils, etc.

The following is a very good way of inserting a counter E.M.F. in any installation:—Let the engine drive a dynamo (let it be called dynamo-regulator) in addition to the generator. If the speed of this dynamo-regulator is very high, and the belt driving it light, long, and slack, and its pulley made heavy to act as a fly-wheel, then this dynamo will have practically a constant speed, even when the engine is not running evenly, *i.e.*, the belt will slip over the pulley sooner than vary its speed, and the power required to drive it will be very small. To run the generator at a constant speed by this method would be too wasteful. Suppose this dynamo-regulator to be placed in "parallel," then it will give a nearly constant counter E.M.F., and this should be arranged to be nearly equal to the normal E.M.F. of the installation. It is evident that any variation in the E.M.F. above the normal, in the main circuit, will be neutralised in the same way as described in the case of the motor-regulator. Such a dynamo-regulator, employed in an installation where a gas-engine is in use for the motive power, and no accumulator, would be found invaluable, rendering the light quite steady: a result almost impossible at present, when only a few lamps are on, and at best there is a slight pulsation in the light. The power absorbed by the dynamo-regulator may approximately be estimated as follows. Suppose the machine to be a "simple shunt," then the whole power required to drive it, so long as the gas-engine runs evenly, is equal to the power absorbed in revolving the armature when producing the F.M. shunt-current. To produce a counter E.M.F., say, of 100 volts in a 100-lamp (20 candle-power) installation would require about 0·5 I.H.P. in a suitably-constructed machine. Hence this is a constant loss all the while the engine is running. Probably this loss of power is set at too high a figure. Now, when a gas-engine is working near full power it runs fairly steady, and there will be but a trivial variable regulating current passing through the dynamo armature; hence the loss of power under this condition will not *be increased* to any practical extent. But when the engine has

only a light load on it, and its speed irregular, then the loss of power, in consequence of the variable regulating current being larger, will be greater, as much possibly as 0·5 I.H.P., so that, when few lamps are in use, the extra power required to steady the light will be about 1 I.H.P. This seems very wasteful, but a great difficulty is overcome: that of getting a perfectly steady light from a gas-engine when only a very few lamps are in use, and the more that are in use at a time the less the loss, till finally the loss becomes very small as compared to the total power required; yet all through the process the benefits of perfect regulation exist.

It will be observed that by all these methods both rise and fall of E.M.F. above or below the normal will be neutralised.

(4) That, if a variable resistance is employed in the shunt circuit of the dynamo, it is possible to charge the cells at any desired rate from the current given at the maximum E.M.F. the dynamo is arranged to give, down to *nil*: and this plan avoids all necessity for altering the speed, and the loss of power is very small. This method has always been employed by the writer, and has many advantages in practice. It is thus possible to work on the lamp circuit and charge at any desired rate at the same time, and in addition any desired E.M.F. may be obtained in the lamp-circuit.

But the condition exists that so long as any current is entering the cells, all the current to the outer circuit is supplied by the dynamo; and this continues until the current to the cells, by means of gradually increasing the shunt-resistance, is regulated down to *nil*, when unstable equilibrium ensues, and the current to the lamp circuit is at a given moment taken half from the dynamo and half from the cells, and this relation will continue for all values of the current within the limits of the installation.* These last remarks require a slight modification in practice, for they neglect the resistance of the leads between the dynamo and the accumulator, any variation of the resistance of the cells, and any difference in the relations between the resistance of the cells

* If more resistance is inserted, then the dynamo runs as a motor, and no longer works as a generator.

and that of the armature. If all these points are taken into consideration, it will be found that the exact proportion of discharge from the dynamo and the cells, instead of being equal, will be somewhat different, but not very much so; and in the writer's installation, after experimenting with many dynamos and two sets of cells, he finds the theoretical division to be very close in practice, the leads having a very small resistance, and the dynamos being fairly suited to the accumulators. Besides other advantages gained by this method, the cells cannot be injured by overcharging or be charged too rapidly.

In short, after the point of unstable equilibrium has been reached, a dynamo and accumulator work to the line very much in the same way as two batteries giving the same E.M.F., placed parallel, would act under similar conditions. It is also seen that, in cases where dynamos requiring much lead for the brushes are used, any decrease of resistance in the outer circuit acts in the same way as putting more resistance into the shunt-circuit, and thus tends to approach the unstable equilibrium point; whereas, when using those machines requiring practically no lead for the brushes, the outer circle may be supplied almost indefinitely (within the capacity of the machine) from the dynamo, charging the accumulator at nearly the same rate all the time.

The author finds that with a "Marshall" steam-engine, fitted with Hartnell's governor, the fluctuations of E.M.F. (standard 100 volts) make but an increased flow of two to three ampères to the cells when the dynamo (Siemens' SD_2 or SSD_{10}) is set to charge the accumulator at the same time as supplying the lamp-circuit, adjusted by means of the shunt-resistance to near balancing point; and further, the nearest approach to unstable equilibrium, so as to work the lamps from the dynamo only, is that of passing three to five ampères to the cells—that is to say, if further resistance is put into the dynamo shunt-circuit, at any moment unstable equilibrium ensues, due to the slight irregularity of the steam-engine. Hence the rise of E.M.F. is limited to one per cent. at the terminals of the dynamo. Many experiments tend to show that in a well-arranged installation, where a gas-*engine is in* use, the E.M.F. at the terminals of the dynamo need

not vary more than $\pm$ 2 volts, and for steam-engine $\pm$ 0·5 volt; these values being measured with no accumulator in circuit.

In order better to understand how the dynamo can charge the accumulator at various rates, and work to the lamp-circuit at the same time, without injury to the lamps, it must be explained that one of the lamp-mains must be capable of being connected (in a 100-volt installation) to the 45th cell during the time that the cells are being charged at the rate of 30 ampères, because 45 cells give 90 to 92 volts and 10 per cent. more, at the time of maximum charging, namely 99 to 101 volts. Again, if the charging is carried on at a lower rate, the lead mentioned must be switched to the 46th or some other cell till, at balancing point, the 49th or 50th is reached. Complicated as this method may appear, nothing can be simpler in practice.

It must be observed that, when charging 50 cells, and one of the lines taken off the 45th cell, the remaining five act as a counter resistance in the lamp-circuit, and these have to pass the total current given by the dynamo. The evil effects of so large a current often flowing through them may be prevented in many ways, such as by creating a shunt to these cells, or having the five cells larger, so as to pass more current, or by coupling these extra cells with an equal number of the others in parallel, etc.

There is another way of looking at an installation worked with an accumulator. Let the lamp circuit be regarded, as a whole, as a variable resistance, and the accumulator as a nearly constant resistance. Then in reality the installation consists of a constant resistance with a variable shunt-resistance (viz., the lamps). This is possibly a new view of the case, and a very convenient one for solving problems.

(5) That the total capacity of any installation equals approximately the total current which may safely be taken from the cells, together with an equal current from the dynamo, if dynamos having but little lead for the brushes are excluded.

This conclusion dictates conclusively the maximum size of dynamo, in point of economy, required in such installations.

(6) That the *results here detailed* may lead to many *modifications in dynamos, by employing* some form of counter E.M.F.

regulator, instead of aiming at regulation in the machine itself, as done at present. The advantage of using this regulator will be that the machine can be made far simpler, with very few coils in the armature, and few plates in the commutator, and if massive soft iron field-magnets are employed there will be no sparking; hence a very simple machine might come into use, with good efficiency, and wound in the simplest way, capable of being used as a direct lighting machine with very good regulation, or for charging accumulators by switching on the cells in the place of the counter E.M.F. regulator.

(7) That the size of accumulator to be used in any particular installation depends on the class of dynamo used, for if its E.M.F. rises rapidly for a decreased flow of current to the outer circuit, then clearly the accummulator may be of smaller size than when the reverse is the case, so that a small accumulator will serve to regulate a large installation if the dynamo is made suitable, that is to say, what would be called a badly regulating dynamo under some circumstances would here be the best machine. Further, the accumulator must be of very large size, if to regulate at all, with dynamos requiring a very small lead for the brushes. In all cases the resistance of the cells must be small compared with that of the lamp-circuit.

(8) That as a general conclusion from the above—

(i.) If dynamos requiring much lead are used, steadiness of light is secured; but it must be observed that the lamps will get a little brighter as the "run" proceeds if the current flowing to the lamp-circuit remains unaltered, because the counter E.M.F. of the cells increases as the charging advances, so that the total current given by the dynamo decreases, and the E.M.F. consequently has a permanent rise.

(ii.) With dynamos having but a very small lead, the E.M.F. in the lamp-circuit remains constant at all periods of the charging, except at those moments when the speed of the machine is irregular and these variations at the terminals of the dynamo are much reduced at the lamps.

(iii.) Lastly, if a dynamo as in case (i.) is employed, and a dynamo- or motor-regulator is used, both defects above mentioned are overcome.

The following is a method for ascertaining what size of accumulator should be chosen for any particular installation:—

Let R = resistance of cells;
„ $n R$ = resistance of lamp-circuit;
„ a = *momentary* variation of E.M.F. above the normal, owing to irregularity in speed;
„ E = normal E.M.F.

Then, if the dynamo is a suitable one, all *momentary* fall of E.M.F. below the normal will be compensated for, and the *momentary* rise of E.M.F. is all that has to be neutralised at the lamps.

Hence, the rise of E.M.F. at the lamps will never exceed $\frac{R}{n R} \times a = \frac{a}{n}$, and the limits of E.M.F. will lie between E and $E + \frac{a}{n}$.

Thus, if a is large, n must be large, or else $\frac{a}{n}$ has a large value, for it is required that $\frac{a}{n}$ shall be made as small as possible.

In all installations the value of a can be ascertained, so that n has to be found. It must then be decided as to how small a variation of E.M.F. is to be allowed at the lamps, and the latitude will depend upon what the light is to be used for; this point settled, the value to be given to n is known.

To find what amount of lead a dynamo should have in any given installation,

Consider only the fall in the speed;

Let k = a constant expressing the percentage of *momentary* fall in the normal speed of engine when not working regularly.

Now, the E.M.F. varies very nearly in direct proportion to the speed, when these are compared at rates not differing much from one another.

Then the varying E.M.F. below the normal may also be expressed by k, hence, to keep the current flowing to the cells *momentarily* constant *for the* decreased speed, the E.M.F. should

be made to rise a percentage equal to k, and this can be accomplished in the construction of the dynamo.

For the lead of the brushes may vary 90°, which may be interpreted, that at 0° the E.M.F. is constant for all values of the current flowing to the outer circuit, speed remaining constant, and close to 90° for a very small difference in the quantity of current taken off, an immense difference in the E.M.F. would follow. The lead can never actually be made to exist at 0° or at 90°, but at any point between. And the *lead* and *rise in the E.M.F.* for a decreased flow of current in the outer circuit vary directly with one another, when compared at close intervals of variation, hence the *extra lead* required to keep the E.M.F. constant at the lamps for any *momentary* fall in current flowing to the outside circuit due to irregularity in the engine is $k \times 90°$. If the usual current given by the dynamo requires a lead of $g \times 90°$, then for this machine to work with cells in any given installation the lead must be made $(g + k)\ 90°$. From this it is seen that g may be made as small as desired, or variable, but k depends on the character of the installation. It may be noticed that $k = a$ in the last investigation.

To obtain the required *extra lead* $k \times 90°$,

Let n = polarity of the armature;

" m = polarity of field-magnets;

" p = angle of lead;

Then $\frac{n}{m} = \sin. p.$

In the case under consideration, p being known, namely, $(g + k)\ 90°$, the relation $\frac{n}{m}$ may be arranged as most convenient.

Returning to a 100-volt installation (50 lamps 20 candle-power) with a gas-engine, if we have $n = 20$ and $a = 2$, then the E.M.F. at the lamps will vary from 100 to 100·1 volts; and if a steam-engine replaces the gas-engine, and $n = 20$ and $a = 0·5$, then the E.M.F. will only vary from 100 to 100·025 volts. Lastly, if it is allowed to vary the E.M.F. as much, say, as one per cent., many combinations may be employed, from a very irregular motor to a very small accumulator. Also the *extra lead* will in *the first case* be 1·8°, and in the second one 0·045°.

It is, perhaps, diverging slightly from the subject to consider the question of starting motors by accumulators, but in many cases the dynamo is made to start the gas-engine which works it in this way, so that the consideration of the question may fairly come here.

On turning the current from the accumulator to the dynamo (having an armature of appreciable resistance)* it is found that great sparking ensues, damaging the brushes and commutator, unless the current is turned on gradually through a resistance, but with machines having very-low-resistance armatures this precaution is unnecessary.

Why this difference? In dynamos whose armatures have a considerable resistance, the flow of current is limited in most cases to within the capacity of the cells, and there will be no appreciable fall of the E.M.F., consequently a strong current in the shunt-coils, hence a strong field, and it is this, combined with a large current flowing in the armature, which creates the mischief if no resistance is inserted at the start.

In the case of a low-resistance armature, more current is permitted to flow from the cells than they are capable of giving, hence a great fall of E.M.F. and a weak field, because very little current passes to the shunt-coils. Therefore the motor starts steadily, and the power rises as the speed increases, the counter E.M.F. in consequence diminishing the current flowing through the armature gradually until the capacity of the accumulator is reached, when the ordinary laws affecting motors come into operation.

In all cases, therefore, it is advisable to start motors with a variable resistance, for in the first instance the dynamo is saved from injury, and in the second one the cells are preserved.

These remarks on motors apply to the majority of cases, since these phenomena occur only where the various apparatus are properly proportioned to one another and the installation.

Thus far have these subjects been examined, and it is hoped to carry the experiments still further, to find some new relations and facts which may prove of service to electricians.

---

* It is these which generally *require* their brushes to have a considerable lead.

The President: Sir David Salomons is very fortunate in having such excellent appliances for trying any experiments he may desire in his unique private workshop, and it is to be hoped that he will be good enough to give the Society, from time to time, the benefits of the results to which such experiments may lead him.

With regard to the system of using accumulators as regulators, the year before last I was trying some experiments with Barnett's secondary batteries, having about 19 in series. They were connected in a Brush arc circuit of a sixteen-light machine—that is, the lead came from the dynamo through a series of arc lights, and then from the last lamp through the accumulators, back to the dynamo machines. A switch was used for cutting the batteries in and out of circuit.

They were thus charged by the current that supplied the arc circuit, and ran the incandescent lamps in five offices and two passages, making in all about 25 lights, most satisfactorily. We found that the light was exceedingly steady, and could not have been better. They acted wonderfully well, because the variation of the current was frequent; but by the system of accumulators a very constant and steady light was obtained. The offices were lighted up about half an hour earlier than the outside lights; and then we ran the lamps from the accumulators only. When the arc lamps were lighted, and the batteries were switched in to the circuit, we saw a slight increase in the light. With this exception, the arrangement worked in every way as satisfactorily as one could wish.

Professor Forbes: There are several points in connection with this paper upon which I should be very glad to have an opportunity of saying a few words.

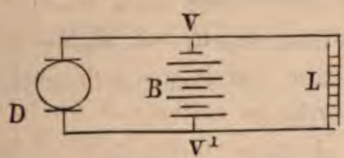
In the first place, I think that most of those who have heard this interesting paper will feel the same regret that I do—that Sir David Salomons, who has, I am sure, spent an enormous amount of time on experiments to get at his results, has given us so very few experimental data in confirmation of these results. The results, no doubt, are perfectly reliable, but we should like to have seen the data put down in figures by which these results

have been arrived at. The most interesting point—to me the newest point—in the whole paper seems to be that which is contained in the second leaf of the proofs, in which he draws attention to the fact that "in simple shunt machines whose brushes require much lead, the E.M.F. rises with a decreased flow of current in the outer circuit. Therefore," he says, "a fall in the speed of the dynamo may be made to raise the E.M.F., and, on the other hand, the E.M.F. may be lowered in the case of a rise in the speed." To me this is a totally new thing. I never heard of it before. I was completely unaware of the fact that you could raise the E.M.F. of a machine by making it go slower. Of course, if the brushes are set for a certain speed, for a speed lower than that at which the dynamo is actually running, then I can conceive that if the speed of the dynamo were to diminish, the fall in E.M.F. would not be so great as it would have been if the brushes had been properly set; it is perhaps even conceivable that the E.M.F. would not vary, or would even rise with a decrease of speed. But in that particular case, if the velocity of the dynamo were to increase, then the E.M.F. would fall off quite as rapidly, even more rapidly, than if it had been in the normal position for that speed. So that this fact is a remarkable thing from a theoretical point of view. It is not, remember, that the E.M.F. will rise when the resistance in the external circuit is increased; it is that the E.M.F. rises when the current is diminished, the outer resistance remaining the same. It is, I repeat, a most remarkable law—so remarkable that I think we are entitled to expect that Sir David Salomons will furnish some tests which bear out and support these conclusions at which he has arrived. It will then be a most interesting addition to our knowledge, and one which deserves a very great deal of attention; but we really must have the figures of the tests before us.

Then a second point is the question of using a dynamo as a shunt on the main circuit. Dr. Hopkinson has already touched on this in a few remarks, and has said, I think with justice, that if we are using a fly-wheel on the regulating dynamo or motor, it would perhaps be equally good to put the fly-wheel on the generating dynamo. There is no doubt that for slight variations,

such as are produced by the joint of a belt in the gearing of the dynamo, putting a motor as a shunt upon it does steady the lights and keep them from blinking. I dare say there are many here who know of previous cases of such a means being adopted. In December, 1881, when I was connected with the lighting of the Scottish Club in Dover Street, I took an opportunity of putting in a motor in that way as a shunt. The lamps up to that time had been flickering most seriously. I put a Gramme machine as a shunt upon the circuit close up to the lamps, and it was perfectly effective: it steadied the lamps completely. The action of the armature of the shunt dynamo completely steadied the lamps and took away the flickering which had been produced by the joint in the belt. I thought myself at the time that this was novel, but I spoke about it to Sir Charles Bright, I remember, and he immediately referred me to a paper in *La Lumière Electrique* of that very week. I really don't know whether it had appeared before I made the trial or not, but M. Marcel Depréz had had exactly the same idea, and it was published at that time. I simply mention this as an instance of where such a thing has been tried before. In my own case I gave it up immediately, on account of the frightful noise it made in the place, and I put in some voltameters instead—that is to say, a number of battery-cells in series, with bits of lead bent over the one into the other, with acidulated water (you might call it a kind of accumulator), and that I found do the same thing with very much less trouble. I may mention that the use of such an arrangement as that, I saw first in the *Times* office. Mr. Macdonald told me of it, and I saw it in January, 1882. I tested it in the Scottish Club, and found it perfectly satisfactory; and I have told a great many friends of it, who have always found it successful. In 1883, when I was at the Vienna Exhibition, I spent a few days at Fiume, where Mr. Whitehead had an installation. They were very much bothered by the flickering of the lights, and I told them of this simple plan. They bent some old lead piping, split, into the shape, and made such a voltameter. Mr. Whitehead wrote to me afterwards, and said it had been perfectly successful.

I think every person who deals with these lamps would be helped enormously by looking at the matter in a numerical point of view. Sir David Salomons told us at the beginning that the paper was to a considerable extent theoretical. I would express the view that I should like to have seen a little more theory in it—that is to say, if he had put down the formula for the current which was passing in the different parts of the circuit, it would help very much indeed to estimate the relative parts which are being played by the accumulator, the dynamo, and the lamps in the circuit. I have never seen in print the statement of these currents in the manner in which accumulators of small resistance do act so perfectly as a regulator, and I wrote it down on the board during the Conference at Philadelphia last autumn; and although there was nothing new in it, yet it seemed that few people had seen it before, and it may be worth while if I just put down a short sketch.



Supposing D to be the dynamo, and at B we have our secondary battery. L is a lamp circuit. The accumulators are placed as a shunt in the lamp circuit. Now we have three different currents passing. A certain amount of current is passing through the dynamo circuits round to points V and V′; a certain amount of current from these points round the lamp-circuit; a certain amount of current between these points through the accumulator. Let us call these different currents by different letters—C_1 the current passing through the dynamo part of the circuit, C_2 the current passing through the accumulator part of the circuit, and C_3 the current passing through the lamp part of the circuit. Again, let E_1 be the E.M.F. which is generated by the dynamo, and let E_2 be the E.M.F. which is generated by the accumulators; and again, let us use the same figures to represent the resistances—that is to say, R_1 is the resistance of the part of the circuit including the dynamo and the leads up to the

accumulator, R_2 is the resistance of the accumulator part of the circuit, and R_3 is the resistance of the lamp-circuit between V and V′. Now let us mark the two points where the accumulator-shunt comes off. Let V and V′ be the potentials at these two points, and let us suppose that the current is positive in any one of these conductors when it flows from V to V′, and negative when flowing from V′ to V. Thus

the current through the dynamo is $C_1 = \dfrac{V-V'-E_1}{R_1}$

" " accumulators is $C_2 = \dfrac{V-V'-E_2}{R_2}$

" " lamps is $C_3 = \dfrac{V-V'}{R_3}$

Now, since the currents flowing from the point V are positive, and those flowing to it are negative, it follows that $C_1+C_2+C_3=0$.

Put in the values of C_1, C_2, and C_3, obtained above, and multiply by $R_1\,R_2\,R_3$, and we have

$$(V-V')\,(R_1\,R_2+R_2\,R_3+R_3\,R_1)-E_1\,.\,R_2\,R_3-E_2\,.\,R_3\,R_1 = 0,$$

and this gives us $V-V'$. We want to know the current flowing through the lamps. This is

$$C_3=\frac{V-V'}{R_3}=\frac{1}{R_3}\cdot\frac{E_1\,.\,R_2\,R_3+E_2\,.\,R_3\,R_1}{R_1\,R_2+R_2\,R_3+R_3\,R_1}=\frac{E_1\,R_2+E_2\,R_1}{R_1\,R_2+R_2\,R_3+R_3\,R_1}$$

This is the general formula which the author should use by inserting the values of E_1, E_2, R_1, R_2, R_3 in each special case. The value of C_3, above, is the current which is passing through the lamp-circuit, under all possible conditions of resistance and electro-motive force in dynamo, accumulators, and lamps. If the resistance of accumulators is small, the equation is simplified; and in the limit where their resistance is very small, it approaches the value given by $R_2 = 0$; *i.e.*, $C_3 = \dfrac{E_2}{R_3}$, and the regulation is perfect, since the current is proportional to the number of lamps.

Professor Adams: I have very little to remark with regard to the paper by Sir D. Salomons. It treats of several very interesting questions, especially the use of accumulators as regulators in an electric circuit, in connection with dynamo machines. The point in which I was specially interested in this paper is that

which is summed up under the last heading on the fifth page, viz., that if a dynamo is employed, and a dynamo and motor regulator is used, then the defects of engines are overcome—that is to say, there is greatly increased steadiness in the electric circuit. No doubt Sir David Salomons is speaking here of the use of continuous-current dynamo machines, because these have been used by him in his electric-lighting experiments. The employment of motor dynamo machines as regulators in connection with an electric circuit is important; and although, as Professor Forbes says, it is not new, yet it has not, I think, been sufficiently dwelt upon or considered hitherto. In my paper on alternate-current machines, given before the Society in November last, I stated, towards the end of the paper, that when three alternate-current dynamo machines were working together to supply an arc light, if one of them be driven as a motor by the other two machines, the three being capable of synchronising with one another, the motor machine had the remarkable effect of steadying the light of the electric arc so that the arc, when the machine was used as a motor, was very much steadier and gave a stronger light than when the machine was not so used. I am alluding to some experiments which I had been carrying out at the South Foreland; and since that paper was read I have been carrying out further experiments on the same machines, three very large De Meritens alternate-current machines. On driving two of these direct from the engine, a very steady light is obtained in a single arc. On driving the whole three machines from the engine, a stronger light is obtained, and it is fairly steady; but on throwing off the belt from one machine altogether, and allowing it to be driven as a motor by means of the current from the other two machines, the light is very much increased, and is rendered very much steadier. The power applied from the engine to the two machines, when these machines work the third machine as a motor and supply the current to the arc, gives a very much better light in the arc than when the whole three machines are driven from the same engine, or when two machines are driven by themselves without introducing the third machine as a motor in the circuit. I had *only* made the experiment once

before the meeting in November, but I have since repeated that experiment over and over again, and always with the same result. The light is much steadier when the third machine is used as a motor than under any other circumstances, the motor acting as a governor in the circuit. In this case I think the result of the experiment shows that a steadier effect is produced in that way than would be produced by means of a fly-wheel, alluded to by Dr. Hopkinson, for these machines are so large that they revolve with the steadiness of a fly-wheel. The mechanical motion is very regular, but the motor acts as a governor of the electric current in the circuit.

Professor AYRTON: The point Sir David Salomons has brought before us is one of great importance, because the flickering of incandescent lamps is not only extremely objectionable to anybody using them, but there is every reason for believing that it tends greatly to shorten their lives, and therefore any means of avoiding flickering not only makes the light pleasanter, but adds to the economy of lighting. I am not quite sure whether Sir David Salomons has specially distinguished in the earlier part of his paper between the two causes of a change of current passing through the incandescent lamps. One cause is attributable to the unsteadiness of the engine, the other to the switching off of lamps in different parts of the building in which they are employed; and in that case you cannot get over that alteration of the current by the method first suggested, *i.e.*, the use of a well-governed engine. For this will not in any way prevent the breaking of lamps when others are switched off in various parts of the building; and I venture to think the whole problem would have been made simpler if the two distinct causes for the variation in the light of the lamps had been separated one from the other.

With reference to the other point which was extremely important, I might ask Sir David Salomons whether in this second paragraph on page 2, "Therefore a fall in the speed of the dynamo may be made to raise the E.M.F.," and so on, is a conclusion obtained experimentally, or deduced theoretically from the first part of the paragraph, "Now in simple shunt machines," etc.

Sir DAVID SALOMONS: Both practically and theoretically.

Professor AYRTON (resuming): Like Professor Forbes, I am a little unable to follow the "therefore." It would be a very difficult matter to deduce theoretically that when you decrease the current in the outside circuit by any possible means, including, for example, diminution of speed of the dynamo, you obtain an increase of E.M.F.; but if this result, as Sir David Salomons assures us, has been obtained experimentally, it is undoubtedly a result of extreme importance.

With reference to changing the number of accumulators, referred to by Sir David Salomons, there were certain devices employed for that object by my colleague and myself, in connection with one of the early uses of accumulators for electric lighting on a somewhat large scale, viz., that at the Grand Hotel, about three years ago, which I do not think have been made public, and which, therefore, it may be interesting to briefly refer to here.

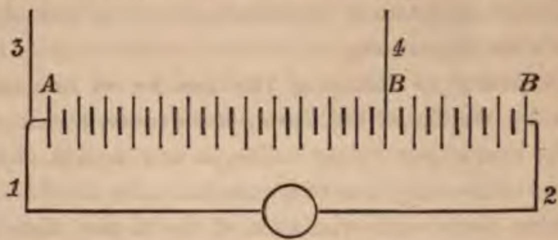
FIG. 1.

During the greater part of the day, the connections of the dynamo were as shown—that is, the cells A B were being charged. At about 3 hours before the lighting commenced, the lead No. 2, Fig. 1, was gradually moved cell by cell, so as to reach the end B′ at about the time the lighting commenced, when the leads from the dynamo would be as shown in 1 and 2 of Fig. 2, and those to the lamps by 3 and 4.

The cells, therefore, from A to B would be discharging, those from B to B′ charging, and the lamps fed by the sum of these two currents. As the cells A B became somewhat exhausted, the wire 4 was moved gradually from B towards B′, so as to reach B′ at about the end of the lighting. Moving from B towards B′ had the effect, of course, of increasing the number of cells discharging, and diminishing the number charging; and, by properly timing the rate of moving the lead 2 coming from the dynamo from

B to B′ previously to the commencement of the lighting, as well as the rate of moving the lead 4 going to the lamps during the lighting, the total amount of energy stored up in each cell previously to and during lighting could be made practically exactly proportional to the amount taken out of it during lighting.

To avoid the successive short-circuiting of each cell as either



Dynamo.
Fig. 2.

the leads 2 and 4 are moved, and which is probably extremely bad for the cells, one or other of four devices may be employed.

First. The contact-blocks C_1, C_2, C_3, permanently attached to the cells, may be farther apart than the width of the sliding contact-maker M, as shown in Fig. 3, so that it cannot rest on two contact-blocks at the same time. This, of course, entirely prevents sparking, but it is generally quite inapplicable, as it necessitates a temporary cessation of the current as it is moved from one contact-block to the next.

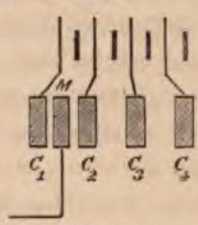


Fig. 3.

Second. Each contact-block may be made double, and the two portions separated by a resistance-coil, as proposed by Sir W. Thomson. With this arrangement, the sliding contact-maker always touches at least one of the contact-blocks, and when it touches two simultaneously, the next cell being added to the *circuit is not* short-circuited, but has its circuit temporarily

closed through a resistance which, although small compared with the total resistance in circuit, is large compared with that of a single accumulator. This arrangement answers well, but has the effect of making the commutator very elaborate and costly, as there must be as many of these small resistance-coils, each of thick German silver wire, as there are accumulators required to be added or substracted from the circuit.

Third. Instead of making each stationary contact-block double, the sliding contact-maker may, as arranged by my colleague and myself, be made double, and the two portions joined by a piece of short thick German silver wire. This is shown symbolically in Fig. 4.

The sliding contact-maker, consisting of the two parts M_1 and M_2, joined by a resistance-coil of a short thick bit of German silver wire, r, is so made that the foremost piece, M_2, touches the

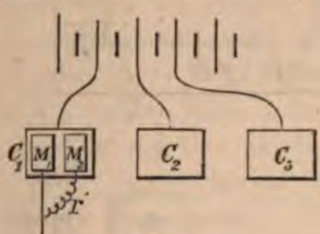


FIG. 4.

second contact-block, C_2, before the other part, M_1, leaves C_1, hence there is never any interruption in the current; and, as the distance between C_1 and C_2 is greater than the width of M_1 or M_2, neither M_1 nor M_2 can rest simultaneously on two stationary contact-blocks, hence an accumulator cannot be short-circuited.

Fourth. But probably the following arrangement that we also employed is still better, since with this arrangement no special resistance-coil is employed.

The room or building in which there are incandescent lamps is wired as shown in Fig. 5, C_1, C_2, and C_3 being the contact-blocks as before, and M_1 and M_2 the two portions of the sliding contact-maker; and it will be seen from Fig. 5 that the resistance-coil connecting M_1 and M_2 is replaced by the whole of one of the leads going to the lamps.

Another device we found necessary at that time to adopt arose from the early forms of the Faure's accumulators not having the very small resistance accumulators have at present, and the necessity, in consequence, of using several sets of cells *in parallel*. Now, if it be desired to add successively groups of cells in parallel, the natural mode of joining them up as is shown in Fig. 6, which represents one end of a collection of cells, A, B, C, D, etc., arranged 3 in parallel, and any number, not shown in the figure, in series. But with this arrangement, if the electro-motive force of any one cell (say, E) for some reason falls below that of either

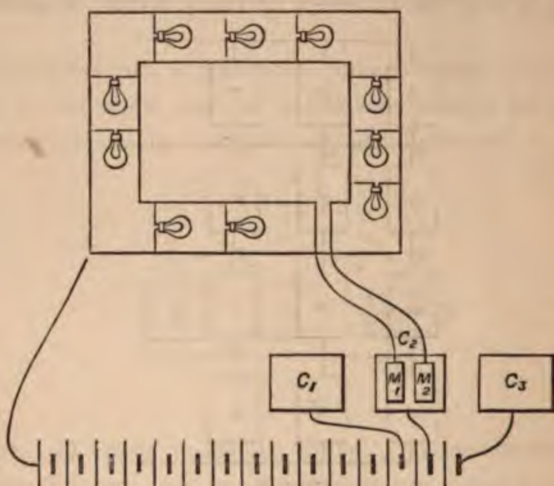


FIG. 5.

of the two cells D or F, in parallel with it, these cells will certainly discharge themselves through E when the cells are standing idle, and may even do this when the cells are being used.

To avoid this happening, it is necessary that the cross-connections, *a b c*, *d e f*, should each only be made when absolutely necessary, and that only one of such cross-connections should be made at one time. This result may be obtained by connecting the corresponding cells in each of the rows A B C, D E F, etc., with consecutive stationary contact-blocks of the commutator, and using a single movable contact-maker which is large enough to touch at the same time as many of the stationary contact-blocks

as there are rows of cells used in parallel circuit. If then the contact-piece be moved on, the additional set of cells, A B C or D E F, is added to or subtracted from each of the rows cell by cell, and at no time is the circuit broken or a cell short-circuited. This arrangement is shown in Figs. 7 and 8, where there are five groups of cells in parallel, and a number, not seen from the figure, in series. P is an arm carrying four contact-pieces which are all electrically connected together, so that they form one large contact-piece, but, in order that the contact shall be good, they are pressed down by independent springs. They touch five contact-blocks, and the arm P is shown in one of the positions it would occupy

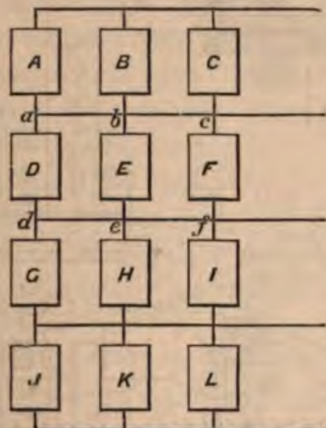


FIG. 6.

while a change was being made in the number of cells in series. If the contact-blocks 1, 2, 3, 4, 5 only were touched by the contact-pieces, all the cells shown in X would be in circuit. When, however, it is 4, 5, 6, 7, and 8 which are being touched, the cells A, B, C are no longer in circuit, the cells D and E being the only remaining ones of the group that are so; and those cells which are not in circuit cannot possibly discharge through one another, as they might otherwise probably do, from small differences in their electro-motive forces.

Hence, although the cells may be charged and discharged in parallel, corresponding cells in the various rows are not connected together, *except when they are* terminal cells of a series, and even

then they are only connected together on one side, so that all wasteful discharging of good cells through others which may have a lower electro-motive force is totally avoided.

I may mention that with all these commutators a locking arrangement for the handle was added, so that it was made impossible to leave a handle in a wrong position. This locking was simply effected by a small roller, carried by the moving handle,

FIG. 7.

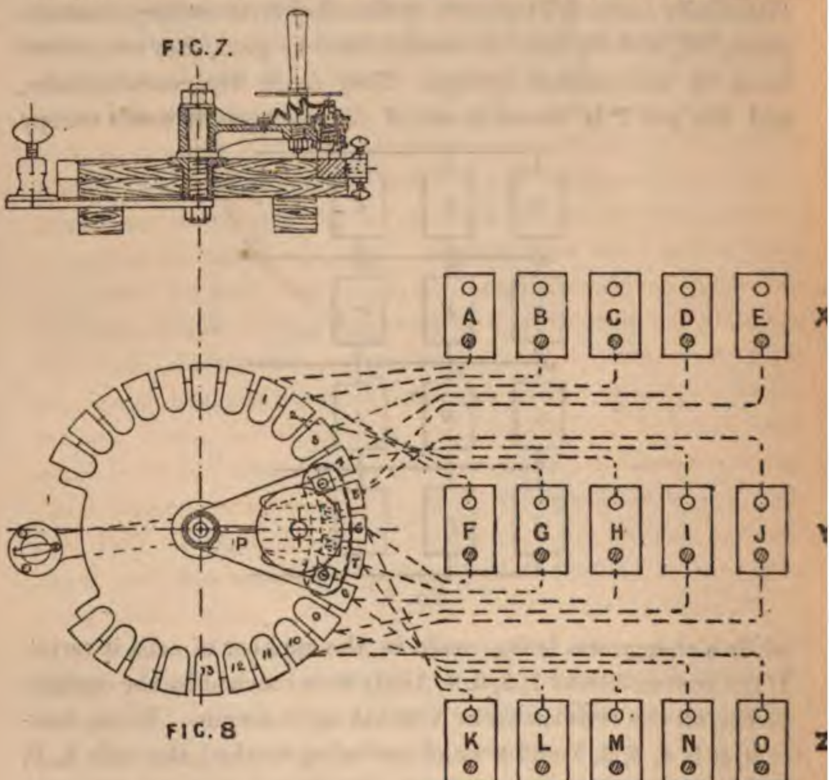


FIG. 8

being pressed down by a spring on a rail formed of a series of hills and hollows, the handle having therefore a strong tendency to move to and rest in a position in which the roller is in the bottom of one of the hollows.

The plan of starting the gas-engine by accumulators, referred to by Sir David Salomons, and which was originally suggested by Sir William Thomson, has been employed regularly by my

colleague and myself. Our experience is not quite that given by Sir David Salomons, namely, that it is mainly with a high-resistance armature that the difficulty occurs in sparking, and that it does not occur with a low-resistance armature. We have used accumulators for starting a gas-engine, which worked both an Edison dynamo and a Bürgin dynamo. The Edison armature had a much lower resistance than the Bürgin armature; but when matters were not properly arranged for the particular case, as much sparking occurred with the one as with the other. The way to get over the difficulty we found to be this. (They were both shunt machines.) The dynamo having to be used as a motor when you are starting the gas-engine, the first thing to be done is to set the brushes of the dynamo for a motor, not for a dynamo, for the lead has to be made quite different for a motor from what it has to be for a dynamo. Then the armature having an extremely small resistance, compared with that of the field-magnets, it follows that if the accumulators be simply connected with the shunt-machine, as ordinarily connected up, there will be an enormous current through the armature, and practically none through the field-magnet as long as the motor has not got up speed. To avoid this we proceeded as follows:—First, the accumulators were attached to the field-magnet-circuit alone, for the purpose of magnetising the field-magnet. Then the armature in series with an auxiliary resistance-coil was introduced parallel with the field-magnet-coil. And, lastly, when the speed had got up to a certain amount, so that the E.M.F. of the motor became equal to that of the accumulators, shown by no current passing either way through the ammeter in the main circuit, the resistance in series with the armature was short-circuited, and the lead of the brushes altered as the machine began to act as a dynamo.

Of course the whole of these alterations could be effected simply by a motion forward of a handle, when it was desired that the accumulators should work the machine as a motor and start the gas-engine; or backwards, when the shunt-machine was to act as a dynamo and charge the accumulators. But time will not allow me to go *further into* the actual construction of these *many devices that we employed.*

Mr. Crompton: This paper is of importance, as it contains much useful information on the subject of working dynamo machines and accumulators in parallel circuit. Such an arrangement seems to be singularly well adapted for the lighting of large country houses; and we are much indebted to Sir David Salomons for bringing before us some of the somewhat difficult problems which present themselves in working out what at first sight would seem to be a very simple matter.

The fact that, if a country house is worked by the direct system (*i.e.*, without the aid of accumulators), it is necessary for the engine-driver or attendant in charge to sit up with the machinery the whole of the time that the light is required in the house, limits such installations to those gentlemen who can afford the luxury of such a special attendant. Granted, therefore, that accumulators are very desirable, there are, as pointed out, two ways of using them. The first and extremely simple plan of working the engine to charge the accumulators during the daytime, so that when they are thoroughly charged the attendant can disconnect the dynamo from the accumulators, switch the latter on to the house-circuit, stop his engine, and go home. For this arrangement the accumulators must be of sufficient capacity to supply sufficient current during the total number of hours that the house is likely to be lighted, and the engine and dynamo must be of sufficient power to charge these accumulators during non-lighting hours.

The second arrangement is that which is the subject of the present paper, in which the engine and dynamo, each of them by themselves capable of doing only half the lighting, are worked parallel, thus considerably reducing the first cost of the whole plant, as compared with that of the first arrangement. Now the merit of the first arrangement is its extreme simplicity, so that it is easy to teach an unskilled person (such as an intelligent under-gardener) to work it; but I think the author has shown conclusively that although the second arrangement will be considerably cheaper to instal in the first instance, yet that the complications thereby introduced are so great that there will be difficulty in providing sufficiently skilled attendance. There are few cases where installations could have such skilled attention as that

described by the author. He himself is a skilled electrician; he is always on the spot, and able to watch the complicated phenomena which he has described this evening. I therefore contend that the difficulties of this problem must be met by some very simple device, and I think that those suggested by the author hardly fulfil this condition.

Before I commence to criticise his methods, I wish to point out in few words the main difficulties we have to meet. As the author has pointed out, it is the best practice to charge your accumulator-cells with a current of E.M.F. considerably exceeding the total E.M.F. of the whole of the cells—that is to say, of the working as well as the reserve cells—when fully charged. Putting this into figures, if we desire to work 110 volt lamps, we require 55 working cells and 5 reserve cells, 60 cells in all; the counter E.M.F. of these being 150 volts. And, when charging accumulators for the first or simple system, we find that although 150 volts would be amply sufficient to drive a fair charging current through the cells during the early portion of the charge, yet when the counter E.M.F. has mounted up to its maximum point, a greater E.M.F. (say, 180 volts) is very desirable, in order to finish the charge quickly. If, therefore, we wish to work on the second or parallel system, we must have a dynamo machine which must work at three different degrees of E.M.F.—at 150, at 180, and finally, if coupled in circuit, to work in parallel with the lamps at 110 volts. One way of meeting this difficulty is to supply an extra large dynamo, having a powerful magnetic field, and by introducing a rheostat into the exciting circuit, to thereby vary the exciting power and strength of the magnetic field, so as to get a sufficiently wide range of E.M.F. given off at the brushes of the machine. It will be seen, however, that this is a somewhat wasteful, and, at any rate, an expensive arrangement. A simpler plan is to vary the speed of the steam- or gas-engine driving the dynamo. This can be done by hand, but far better automatically by means of an electric governor. Mr. Willans, of Thames Ditton, has succeeded in perfecting an electric governor for driving steam-engines, which can be arranged, as desired, either *to maintain a constant* E.M.F. between two points

in the circuit or a constant current through the main conductors. In the simple arrangement of charging the accumulators separately, a governor arranged on the latter principle would be most suitable. The steam-engine would be automatically maintained at a low rate of speed during the early part of the charge, and at a somewhat higher speed during the latter part of the charge, the charging current remaining constant throughout; but for the parallel arrangement, the governor, which maintains a constant E.M.F. between any two points in the circuit, would have to be used. If in Sir David Salomons's case the two ends of the solenoid circuit of this governor were connected on to the mains at a somewhat central position in the house (speaking electrically), —that is to say, to such a point that the fall of E.M.F. in the various branches due to their resistances would be as near as possible equal,—then I think that this governor would of itself do all that the author requires. The engine and the dynamo would be automatically driven at the speed best suited to supply to the accumulators and to the house-circuits respectively, each such a fair share of current that the central point above mentioned would be maintained at the desired E.M.F.; and I am hopeful to be able to persuade the author to make a trial of a governor arranged on this plan.

I must now call your attention to the result of the author's observations on the effect of the angle of lead of the brushes of dynamos in connection with this matter of parallel working. If further experiments prove the author's assertion that we ought to return to the use of dynamos having a large angle of lead of brushes, then I must say I think the prospect is extremely discouraging to myself and to the other labourers in this field who have been doing our utmost to reduce this angle of lead by enormously increasing the strength of the magnetic field, and by reducing the self-induction of the armature coils themselves. The author, to prove his point, has compared the performances of one of Messrs. Siemens's machines with one of our make. There is no doubt that the experiment, as conducted, showed conclusively that the charging current from the dynamo to the cells showed greater *pulsations in the* case of our machine than in that of Messrs.

Siemens; and I think I can readily point out the cause of this. I should here apologise to the author for not having informed him previous to the meeting of what I am now going to say; but, unfortunately, I was unable to examine the characteristic curve of the machine made by us until quite recently, and after the paper was issued. His experiments proceed on the assumption that both machines are worked in their normal condition of strength of magnetic field. Dr. Hopkinson has pointed out, here and elsewhere, that there is a particular point in the characteristic curve of every machine at which a very small increase in the exciting current produces a great difference in the strength of the field, and hence a vast difference of the E.M.F. given by the armature. He calls this point the critical point. Messrs. Siemens's machine was a pure shunt machine, with comparatively little iron in the field-magnets, which were therefore tolerably well saturated. Our machine, on the contrary, was a compound machine, having the thick series-coils wound *next* the iron of the field-magnets, the shunt-coils being outside them. At the normal rate of output at the terminals, little more than half the exciting power is given by the shunt-coils, the remainder being given by the series-coils. At the time the author carried out his experiments the series-coils were thrown out of action, and the field was given entirely by the shunt-coils. The field was therefore a very weak one,—in fact so weak that it was well on the critical part of the characteristic,—and the E.M.F. and current were in a state of extreme instability; consequently every slight irregularity in speed caused by the beat of the engine was exaggerated in the pulsation of the E.M.F. and current. I will not trespass further on your time, as I think I have said enough to show that the author, to prove his point, must at any rate work the two machines under condition of equal saturation of the field-magnets.

It would be extremely undesirable to return to the use of machines having a large angle of lead. A large angle of lead means an angle which varies with the amount of current passing through the armature; in other words, with the output of the machine. Therefore a machine so made requires constant attention to keep the brushes in the right angular position to ensure them in the minimum of sparking.

I wish to bring comfort to the minds of those who are using, or about to use gas-engines, as the means of driving their electric machinery. The author has, I think, considerably exaggerated the difficulty in getting steady lighting when gas-engines are used. I myself am confident that if proper precautions are used there is no difficulty in working incandescent lamps direct from a dynamo driven by a gas-engine, without any appreciable unsteadiness or winking of the lights; all that is required is that the dynamo should run at a high speed, and have a heavy steel fly-wheel, the periphery of which should run 10,000 feet, or nearly two miles, per minute. A fairly long and slack belt, giving about 5 or 6 per cent. slip, should be used. My opinion is corroborated by Mr. Holt, of Crossley's, whose experience with gas-engines is second to none. Mr. Holt states that with the above percentage of slip no eye can detect the variation in the light due to the beat of the engine. This 6 per cent. slip does not mean 6 per cent. loss of power. The loss is very much less. If we take the case of the gas-engine which I use at my own house, the gas required to drive the engine and dynamo empty is about 60 cubic feet per hour; when running with a full load of 35 lights, it uses 130 cubic feet per hour. At this time the loss caused by the slip is about 1¾ per cent.; but if we take the case when the engine is very lightly loaded,—that is to say, when she is taking 80 cubic feet per hour,—the loss, slightly increased, comes up to about 3 per cent.

The method of cross-excitatation described by Dr. Hopkinson has already been applied in several cases. Mr. Gisbert Kapp tried it under my own directions, at Lord Randolph Churchill's, and it produced the desired effect: the difficulty was this—that, as at that time the accumulators were not very perfect, any failure or short-circuits in the cells that were connected with the field-magnets circuit of the machine had an exaggerated effect on the rest of the circuit, and on this account we were compelled to give it up.

Dr. J. A. Fleming said that, although many methods of regulating E.M.F. had been brought before them in the paper and discussion, he had failed to hear mention of one which, as a device of Mr. Edison's, deserved a moment's notice. In this plan a high-resistance relay was placed across the circuits, the

armature lever working between two stops. When the E.M.F. in the circuit was too high, it pulled the armature over against one stop and closed a local circuit which operated a motor so arranged as to move gearing which introduced resistance into the field-circuits. By this means the magnetic fields of the dynamos were diminished in strength, and the E.M.F. brought down to its normal amount. If the E.M.F. fell below this, the relay armature was pulled over by a spring against the opposite stop, and closed another circuit which operated a reverse motor and took resistance out of the field-circuits. Although the device acted well enough to meet the case when lamps were steadily thrown in or out of circuit, it did not respond readily enough to meet or cover sudden variations in speed in the engine. It had been found preferable in New York to abandon the relay and motors, and substitute an attendant and a galvanometer. This proceeding reminded him of the experience of an astronomer who, having failed to obtain a perfect heliostat worked by mechanism, had at last substituted, with advantage, for clock-work a little boy; and it was not improbable that the best method of regulating in central stations would be found to be to abandon mechanism, and govern by resistance introduced or removed from the field-circuits by the aid of a delicate voltmeter and a little boy.

Mr. George S. Corlett: First of all, I ought to explain that I represent Mr. Sydney Walker, of Cardiff, who is unable to attend. On his behalf I should like to offer one or two remarks on the paper we have heard this evening.

It seems to me that, next to steadiness of light, simplicity and economy are the most important considerations. In many instances the complication and expense involved by the use of either accumulators or a second dynamo would be an effectual bar to their introduction.

Mr. Walker has invented, and his firm have worked out, two remarkably simple automatic regulating apparatus, which it is believed will do all that is required towards maintaining the current and E.M.F. constant, without having recourse either to accumulators or an additional machine. These instruments, models of *which lie on the table*, have been subjected to a number

of experimental tests, and from the data thereby obtained the above conclusion is arrived at.

The first regulator, which is also designed to take the place of fusible cut-outs, opens an additional path of variable resistance to the circuit, allowing of the passage of a portion of the current on the arrival of an excess E.M.F.

An electro-magnet, which can be connected either in circuit with the work or in derivation, and may be constructed either with a solid core or in the solenoid form, has, facing its pole and carrying its armature, a lever pivoted at some convenient point. At one end of this lever is an adjustable spring, the other carries a carbon point. Opposite this latter is fixed a second carbon point, to which is connected an adjustable resistance such as an incandescent lamp.

On the arrival of an E.M.F. slightly above the normal, the electro-magnet, becoming more powerful, pulls the movable carbon into light contact with the fixed one, thereby opening a very-high-resistance shunt to the circuit, since the carbon points are not firmly connected and so offer a considerable resistance, and the lamp, as only a small current is passing, will have its cold resistance, or very nearly so. Should the E.M.F. still further increase, or a greater one arrive, the magnet pulls the carbons into firmer contact, thus lowering their resistance, and as now a larger current is passing through the shunt, the lamp being warmed, its resistance will be very considerably reduced.

When the E.M.F. falls off again, the reverse takes place. The magnet, being weaker, presses the carbon points less firmly against each other, thus throwing in the shunt path an increased resistance, so decreasing the quantity of current passing through the incandescent lamp, and thus increasing its resistance, and so as the E.M.F. falls to the normal, the carbons are in less and less firm contact, till when the E.M.F. reaches the normal the carbon points are disconnected.

The ever-varying changes in a brush circuit are very beautifully shown by this instrument.

It will be seen that the regulation of the apparatus is unde *perfect control*, and can be multiplied *ad infinitum*.

The tension of the spring can be adjusted so as to pull up at any desired percentage of increase. The carbon contacts may be considerably varied, or they may compress a column of carbon dust. Also the number of incandescent lamps may be increased.

Two regulators may be set to work, one against the other, one pulling up at, say, from 1 per cent. to 5 per cent., and the other from 5 per cent. to 10 per cent., and so on.

The second regulator is very much the same as the one I have briefly described, except that in this case the carbons are reversed and form part of the circuit to be regulated. We have the electro-magnet, lever, adjustable spring, and carbon contacts. The spring opposes the action of the magnet and tends to keep the carbons in hard contact. On the arrival of an increased E.M.F. the magnet tends to separate the carbons, thus throwing an artificial resistance into the circuit. This form of regulator, which can be made very sensitive, is well adapted for installations where the motive power is a gas-engine, as, despite the excellent results detailed by Mr. R. E. Crompton, gas-engines, putting it in a mild form, are occasionally known to vary their speed.

Though in this instrument it has not been thought necessary to provide for a lowering of E.M.F., yet it will readily be seen that a very simple arrangement, such as allowing a spring to compress a column of carbon dust or a series of plates, will do all that is required in that direction.

These regulators can be used at any part of the circuit, and, if desired, any number can be inserted at different places. They also provide against such accidents as the engine running away, etc., and also take the place of fusible cut-outs, which, if electricity is to become a household matter, laid on as gas is at present, will hardly come into general use. People will not care to have all their lights cut off at a moment's notice, on account of some slight fault outside or at the machine.

Sir David Salomons: I am pleased to find that my paper has been so favouraby received, and in reality there are but two speakers who have attempted to make a hit against any of the points therein contained. However, I will consider each speaker in his turn, and attempt to reply satisfactorily to every point raised.

I do not deem that any further remarks are required on my part in answer to the observations made by your President, Dr. Hopkinson, Professor Grylls Adams, and one or two other speakers. I feel it, however, desirable to reply fully to Professor Forbes, Mr. Crompton, and Professor Ayrton.

Many speakers have referred to a point in my paper which says that for a fall of the speed in the dynamo the E.M.F. rises. This must not be taken literally by itself. It must not be supposed that there is a sudden variation of the E.M.F. for a sudden variation in the speed of the dynamo, but that these processes go on continuously and gradually; and it will be noted that throughout the paper the changes are spoken of as *momentary*. Therefore, when the speed falls, and the E.M.F. with it, and the current is consequently diminished, there is a momentary rise of E.M.F., and before any further fall occurs the irregularity in the motion of the motor has probably vanished, and all this before time will allow the temperature of the lamp filaments to vary appreciably. It is possible that regulation by means of a motor is not as completely novel as I imagined it to be. Still, I believe that no one hitherto has given any special attention to the subject, and worked it out with a view of bringing it into practical use. Some speakers have stated that fly-wheel power in itself would be sufficient to obtain steadiness from an irregular motor. The loss, however, would be too great, were this method employed except in very small installations. Some speakers have already replied to a few objections which have been raised, so I will only confine myself to such points as have not been cleared up already.

Professor Forbes has given you an elaborate set of formula which are decidedly interesting, but equally unpractical. Theoretically, I admit them, every one; but in practice they cannot be conveniently applied, and my very object has been to place before you a practical method for ascertaining various results where accumulators are used in an electric-light installation. And I think if you were to look through my paper again carefully, you would find that this has been fairly met. This gentleman also refers to an apparatus which he terms "*voltameter*," and I hope on some future occasion that we shall

hear more about it, for at present I cannot see its application except in a very small installation. Many gentlemen have referred to their having observed, in cases where two dynamos are working in parallel, that if one should give a higher E.M.F. than the other, causing it therefore to work as a motor, certain advantages were at once obtained. I must here point out that, unless the conditions which I have mentioned in the paper exist, the regulation spoken of cannot exist—that is to say, that the speed of a dynamo running as a motor must be absolutely constant. A great deal has been said this evening against the use of an accumulator—that so many methods now exist by which perfect steadiness may be obtained without their use. Let me ask you, then, why should there be now such a much greater activity in the market for accumulators simply to use as regulators, and not for use as storage, if the methods spoken of were so very perfect? I admit that accumulators are not desirable in very large installations, such as might exist for the public supply of the electric light, but except in these cases their use is extremely desirable, and when employed for regulating only there is practically no loss of power.

Professor Ayrton has referred to that portion of my paper dealing with the starting of the gas-engine by means of the cells. Although he appears to doubt my remarks, I must point out that he really confirms me—that is to say, he states that, by shifting the brushes and inserting a resistance, sparking may be prevented at the start. This is true enough, but it must be borne in mind that dynamos whose armatures have a very small resistance—namely, whose brushes require a very small lead (as at present constructed)—have the lead practically set to run as motor or dynamo for a short time, and therefore do not perceptibly spark at starting. It must also be remembered that electrical currents act very much in the same way as what is termed "force" on matter, and that such a thing as electrical inertia may be said to exist; and some writers make use of this expression. Professor Ayrton has further shown you, although a little apart from the matter in hand this evening, some excellent ways of shifting cells in and *out of* circuit, without placing any

on short-circuit. Let me also call your attention to an excellent switch made by Messrs. Siemens, where the last break is made off two carbon pencils, so that the metal part of the switch is in no way burnt; and if the carbon pencils are made of proper length it is possible that they may even be used for inserting a resistance in the circuit as well.

Mr. Crompton is rather hard on the use of accumulators, for he says that they are only essential in all country-house installations, unless a man is a Crœsus, and can afford to keep a man running the engine all day and all night, so that at no time should it be possible that a single lamp anywhere cannot be lit. But I can testify to the fact that if two men were employed, one for day-work and one for night, it would be cheaper than accumulators; because at those hours when few lights only are likely to be required a very small engine and dynamo could be kept running, and the larger installation put to work during the grand lighting hours. Now the dynamo-regulator and motor-regulator of which I have spoken, you must easily see, can be applied with great facility in most factories, where the trouble of having an accumulator is considered undesirable, and where the electrical governor would also be undesirable, as it might upset the general machinery in the establishment—in other words, perhaps an engine of 500 horse-power would have to be controlled by an electrical governor for the sake of running, say, five or six arc lights. I agree that an electrical governor is an excellent apparatus, although a delicate one, but that where accumulators are used it is not always desirable to have one in action. I have had the satisfaction of seeing his installation, although I had no opportunity at the time of testing carefully the steadiness of the light; but apparently it was steady. I use an extremely severe test, which is perhaps more delicate than any photometer or other instrument at present in use—*i.e.*, take a piece of white paper with some writing on it, lit up by the lamps which have to be tested, and stare very hard at it. You will at once observe the slightest flicker in the light. The action is practically as follows:—The effort of staring at the paper is an *attempt to fix* the iris of the eye, and any variation in the

light causes this organ to feel the change, because of the natural tendency for the opening to alter its diameter. It has been pointed out that the Crompton 14-unit machine is a compound machine, with the series wire cut out so that the field-magnets are only excited at a point very much below what they should be. This may be so or may not, for this machine was built to special order, so that there should be sufficient shunt wire on the coils that it might act as a pure shunt when required, but that when working compound a resistance of eleven or twelve ohms must be inserted. I therefore believe that when working as a simple shunt the field-magnets are fairly excited. The Siemens machine referred to was built about the end of 1884, so that it cannot be described as an old-fashioned machine, although certainly it may come under the denomination of an old type; and although I would give the prize for non-sparking to the Crompton machine (for when the brushes are properly set there is absolutely no sign of it), still the Siemens machine runs with scarcely any appreciable sparking from a few ampères to over 100. Hence there is no necessity to be so very harsh on this type.

A few objections have been raised to my method of placing variable resistances in the shunt-circuit, as likely to destroy the proportions of the machine and cause it to work badly, either by sparking at the brushes, or by destroying its curve, or in some other manner. This in practice is, however, quite erroneous, for the amount of resistance which is inserted to obtain the desired end is extremely small as compared with the resistance of the shunt-coils.

I believe that I have now replied to all the points which have been raised by the various speakers, and hope that the replies are satisfactory in your judgment.

A ballot then took place, at which the following were elected, and the meeting was adjourned until 26th March :—

Associates :

Arthur Wilfred Brewtnall.	Julius Maier, Ph.D.

Students :

Oswald M. Andrews.	Frederick W. Anthony Knight.
Edwin John *Merlet Clapham.*	Robert Walter Underhill.

THE LIBRARY.

ACCESSIONS TO THE LIBRARY FROM FEB. 20 TO MARCH 25, 1885.

By Alfred J. Frost, *Librarian.*

(*Works marked thus* (*) *have been purchased.*)

IT IS PARTICULARLY REQUESTED THAT MEMBERS WILL PRESENT COPIES OF THEIR WORKS TO THE LIBRARY AS SOON AS POSSIBLE AFTER PUBLICATION.

Blavier [E. E.] Des Grandeurs Electriques et de leur mesure en unités absolues. Comparison des unités Électrostatiques et des unités Électro-magnétiques. 8vo. 41 pp. [*Annales Télegraphiques, Troisième Série.* Tome Sixième, Nov.-Dec., 1879, p. 515.] *Paris,* 1879

Blyth [Jas.] [*Vide* Ferguson, Robt. M.]

* **Delahaye** [Ph.] L'Année Électrique, ou exposé annuel des travaux scientifiques des inventions et des principales applications de l'électricité à l'industrie et aux arts. 12mo. xii. + 312 pp. *Paris,* 1885

Edgeworth [R. L.] A Letter to the Rt. Hon. the Earl of Claremont on the Tellograph, and on the Defence of Ireland. 8vo. 54 pp. *London,* 1797
[Presented by Latimer Clark, Esq., Past-President.]

* **Ferguson** [Robt. M.] Electricity. New edition. Revised and extended by Jas. Blyth, M.A. 12mo. 410 pp. *London and Edinburgh,* 1883

Fontaine. [*Vide* Niaudet.]

* **Glazebrook** [R. T.] **and Shaw** [W. N.] Practical Physics. 12mo. 487 pp. *London,* 1885

Hospitalier. [*Vide* Niaudet.]

Hughes [Prof. D. E.] [*Vide* Thompson, Silvanus.]

Indian Telegraphs. Administrative Report of the Director-General of Telegraphs in India, of the Indian Telegraph Department for 1883-84. Fo. 43 pp. *Calcutta,* 1884

Institution of Civil Engineers. Name-Index. Vols. I. to LVIII. of Minutes of Proceedings. Sessions 1837 to 1878-79. 8vo. 489 pp. *London,* 1885

Ishie [T.] [*Vide* Japanese Telegraphs.]

* **Jaite** [G.] Der Telegraph und der Automatische Umschalter. 8vo. 19 pp. Plates. [*Dingler's Polytech. Jl.,* 1875. Bd. 216.] *Augsburg,* 1875

Japanese Telegraphs. Ninth Report of the Director-General (T. Ishie) of the Imperial Government Telegraphs for the Fiscal Year ended June 30, 1883. 8vo. 24 pp. (Received March, 1885.) *Tokio,* n.d.

Kilner [Walter J.], M.B., The Effects of Electricity upon the Circulation, local and general. 8vo. 10 pp. [Reprinted from Vol. XIII. of *St. Thomas's Hospital Reports.*] *London,* 1885 ?

Mascart *[M. E.]* Traité d'Électricité Statique. 2 vols. 8vo. *Paris,* 1876

Melsens. [*Vide* Pearse.]

* **Niaudet** [A.] Traité élémentaire de la pile Électrique. Troisième edition revue par Hippolyte Fontaine, et suivi d'une notice sur les accumulateurs par E. Hospitalier. La. 8vo. xv. + 353 pp. *Paris*, 1885

* **Pasley** [Lt.-Col.] Practical Rules for making Telegraphic Signals, with a Description of the Two-armed Telegraph invented in 1804 by Lt.-Col. Pasley, R.E. 8vo. 59 pp. [Lithographed at the Establishment for Field Instruction, Royal Engineer Department, Chatham.] *Chatham*, 1822

* —— Observations on Nocturnal Signals in general, with a simple method of converting Lt.-Col. Pasley's Two-armed Telegraph into a Universal Telegraph for day and night signals. 8vo 53 pp. [Lithographed at the Establishment for Field Instruction, Royal Engineer Department, Chatham.] *Chatham*, 1823

Patent Office. The Illustrated Journal of Patented Inventions La. 8vo. [Commenced Nov. 14, 1884.] *London*, 1884-5

Pearse [J. W.] Lightning Conductors on the Melsens System. 8vo. 14 pp. *London.*

Senlecq [C] Le Télectroscope. Appareil destiné à transmettre à distance les images par l'électricité, basé sur la résistance conductrice variable du Selenium aux différentes gradations de lumière. 8vo. 36 pp. Plate. *Paris*, 1881

Shaw [W. N.] [*Vide* Glazebrook and Shaw.]

Smith [Willoughby]. Induction and Conduction. 8vo. 21 pp. *London*, 1885

Thompson [Prof. Silvanus P.], B.A., D.Sc. On Interference Fringes within the Nicol prism. 8vo. 9 pp. [*Proc. Physical Soc.*, Vol. II.] *London*, 1877

—— On Binaural Audition. Part I. 8vo. 4 pp. [*Phil. Mag.*, Oct.] *London*, 1877

—— Magnetic Figures illustrating Electro-dynamic Relations. 8vo. 6 pp. 2 plates. [*Phil. Mag.*, Nov., p. 348.] *London*, 1878

—— Action of Magnets on Mobile Conductors of Currents. 8vo. 6 pp. Plate. [*Phil. Mag. (Supplement)*, Dec., p. 505.] *London*, 1879

—— Notes on the Construction of the Photophone. 8vo. 6 pp. [*Phil. Mag.*, April, p. 286.] *London*, 1881

—— Optical Illusions of Motion. 8vo. 10 pp. [Reprinted from *Brain*, Part XI.]

—— On Volta-electric Inversion. 8vo. 5 pp.

—— On the Double Iodide of Mercury and Copper. 8vo. 1 p.

—— On a New Polarising Prism. 8vo. 3 pp. [*Phil. Mag.*, Nov., p. 349.] *London*, 1881

—— On the Opacity of Tourmaline Crystals. 8vo. 18 pp. [*Phil. Mag.*, Aug., p. 112.] *London*, 1881

—— On the Conservation of Electricity, and the Absolute Scale of Electric Potential. 8vo. 13 pp. [*Phil. Mag.*, July, p. 13.] *London*, 1881

—— On the Function of the Two Ears in the Perception of Space. 8vo. 11 pp. [*Phil. Mag.*, June, p. 406.] *London*, 1882

—— On the Graphic Representation of the Law of Efficiency of an Electric Motor. 8vo. 8 pp. *Phil. Mag.*, Feb., p. 124.] London, 1883

Thompson [Prof. Silvanus P.], B.A., D.Sc. On the Adjustment of Resistance Coils. 8vo. 6 pp. [*Phil. Mag.*, April, p. 265.] *London*, 1884

——— On a Modified Resistance-Balance. 8vo. 3 pp. Plate. [*Phil. Mag.* (*Supplement*), June, p. 507.] *London*, 1884

——— Note on the Theory of the Magnetic Balance of Hughes. 8vo. 13 pp. [*Proc. Roy. Soc.*, No. 230, p. 319.] *London*, 1884

——— On a New Insulating Support. 8vo. 2 pp. Plate. [*Phil. Mag.*, Feb., p. 134.] *London*, 1884

Thomson [Sir W.], F.R.S. Mathematical and Physical Papers. Vol. II. Collected from different Scientific Periodicals from May, 1841, to the present time. 8vo. 407 pp. *Cambridge*, 1884

Walker [Sydney F.] On the Principles of Electric Lighting, and the Construction and Arrangement of Electric Light Apparatus. La. 8vo 62 pp. Plates. [*Proc. of the North of England Institute of Mining and Mechanical Engineers*, Vol. XXXIV., 1884.] *Newcastle-on-Tyne*, 1885

To Bal
co
" Less
Ex
" Sub
"
"
" Entr
"
" Life
" Publ
" Sale
" Profi
" Divid
" Divid
" Adve

ORIGINAL COMMUNICATION.

By Thomas H. Blakesley.

Some short time ago, in the discussion upon Dr. Hopkinson's paper upon alternating currents, I gave the geometrical solution of a problem, into which, unfortunately, an error was unwittingly introduced. The problem related to the action of a condenser employed to bridge a circuit in which a generator of harmonically varying electro-motive force is at work, in modifying the resulting currents.

The circuit is divided by the poles of the condenser into two portions, one containing the generator, the other called the remote section.

Each section has its own proper resistance, and may have its own coefficient of self-induction.

So far as the cases in which the remote section has no coefficient of self-induction are concerned, the constructions given on the occasion mentioned are correct; but when there is a coefficient of self-induction in the remote section, a modification is required, which I desire to furnish in correction of the construction then given. The problem may be succinctly stated as follows:—

A generator of a harmonically varying electro-motive force of period $2\,T$ operates in a circuit of resistance $\overline{r + R}$, r including the generator, and R being the remote section. At the junctions of the sections are situated the poles of a condenser of capacity C. It is required to give a geometrical construction showing the relative magnitudes of the three electro-motive forces, viz., the effective electro-motive forces in each section and the electro-motive force of the generator, when the sections represented by r and R respectively have coefficients of self-induction of the value l and L respectively; and also the relative phases in which these electro-motive *forces are related* to each other.

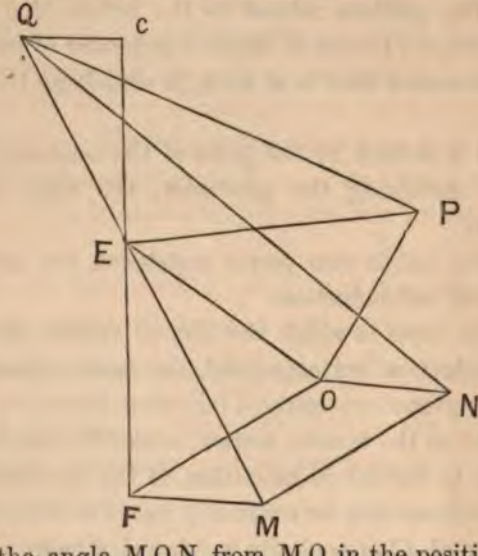
Take any straight line, E C, to represent the effective electromotive force in the remote section, R, when at a maximum.

Set off the angle C E Q from C E in the direction opposed to the motion of the hands of a watch, which call the positive direction, such that the tangent of C E Q is equal to the numerical quantity $\frac{L\,\pi}{T\,R}$, of which each factor is known, and draw C Q perpendicular to E C.

Produce C E, Q E to F and M respectively, so that

$$CE : EF :: QE : EM :: R : r.$$

Join M F, which will be parallel to C Q.



Set off the angle M Q N from M Q in the positive direction, such that the tangent of M Q N is equal to the numerical quantity $\frac{C\,\pi\,R\,r}{T\,(R + r)}$, of which each factor is known, and draw M N perpendicular to Q M.

Complete the parallelogram N M F O, and join O E.

Set off the angle O E P from O E in the positive direction, such that the tangent of O E P is equal to the numerical quantity $\frac{l\,\pi}{T\,r}$, of which each factor is known, and draw O P perpendicular *to E O.*

Finally, join Q P.

Then

P Q is the electro-motive force of the generator when at a maximum,

and

O E is the effective electro-motive force in the generating section when at a maximum,

both on the same scale in which E C represents the effective electro-motive force in the remote section when at a maximum.

If the diagram is made to revolve in the positive direction through the four right angles in the period 2 T at a uniform rate, the projections of these three straight lines upon any one fixed straight line will represent the actual electro-motive forces *at any moment.* Their position in the diagram will therefore also represent their phases at any moment with respect to any fixed straight line, and therefore with respect to each other.

On the same scale and plan, E Q represents the electro-motive force constituted by the difference of potential of the poles of the condenser.

The maximum *currents* in the two sections are equal to the effective electro-motive forces respectively divided by the resistances r and R, *i.e.*, to $\frac{\mathrm{O\,E}}{r}$ and $\frac{\mathrm{E\,C}}{\mathrm{R}}$, which values are proportional to O E and F E respectively, the resultant current charging the condenser being represented on the same scale by O F, its real value being $\frac{\mathrm{O\,F}}{r}$.

This line O F must be at right angles to E Q, the electro-motive force between the plates of the condenser.

ABSTRACTS.

(We are indebted to the courtesy of the Institution of Civil Engineers for allowing the first five of the following Abstracts to be reprinted from their Proceedings.)

A. LEDUC—HALL'S PHENOMENON.

(Comptes Rendus, T. 98, p. 673, 1884.)

If there is placed between the poles of a powerful electro-magnet, and perpendicular to the lines of magnetic force, a very thin metallic plate traversed by a current, there is developed in this plate a transversal electro-motive force, which Mr. Hall considers due to a pressure sustained by the current. Mr. Righi has found that the effect is much greater in bismuth than in the other metals hitherto studied. The author now announces that he finds that the transversal electro-motive depends on the quantity of current that traverses the plate; on its temperature; on the mean magnetic intensity in the space that it occupies in the field. This is true for bismuth. For silver there is a deflection of the lines of equipotential dependent on the temperature. If the magnetic intensity does not exceed a certain value, the deflection of the line of equipotential and of the lines of force at the points where they intersect is represented by $D = k M (1 - a t)$, k being the deflection produced at the temperature 0° at a point where the magnetic intensity is equal to 1. a is another constant, and for bismuth is very small; for silver it varies from 0·008 to 0·009. This deflection may be considered due to the heterotrophy that the metal takes in the magnetic field analogous to that sustained by light in falling normally on a bi-refracting substance.

The phenomenon is very feeble in an alloy of bismuth and lead (equal weights); and, according to Mr. Hall, it is nil in lead. The crystalline state of bismuth, therefore, appears to play a great part in the production of the phenomenon.

P. LUCAS—RESISTANCE OF CARBONS EMPLOYED IN ELECTRIC LIGHTHOUSES.

(Comptes Rendus, T. 98, p. 800, 1884.)

The Carré carbons employed in the French lighthouses are 0·016 mètre in diameter, as used for the ordinary lights, and 0·024 mètre for the "double" lights used when there are fogs. The resistance at 15° centigrade is, as nearly as may be estimated from a large number of bridge-measurements, 70 ohms per square millimètre of section and per mètre length, but the differences *from this* average amount to as much as 30 per cent. The author has made

experiments to ascertain how these carbons vary in resistance with the quantity of electric current. The results led to the empirical formula

$$y = y_0 \left(1 - \frac{\mathrm{I}}{25 \text{ ampères} + 1\cdot 2\ \mathrm{I}}\right),$$

y_0 being the resistance at 15° centigrade, and the quantity of current I varying from 50 to 142 ampères, when the coefficient of y_0 diminished from one-third to one-fourth.

Further measurements as to the variation of temperature led to the formula

$$\mathrm{T} - 15° = \frac{\mathrm{I}}{0\cdot 112 \text{ ampère} + 0\cdot 0004\ \mathrm{I}};$$

and $y = y_0 \dfrac{1 + 0\cdot 0005\,(\mathrm{T} - 15)}{1 + 0\cdot 0005\,(\mathrm{T} - 15)}$, which may be applied for temperatures between 400° centigrade and 900° centigrade.

Putting $\theta = \mathrm{T} - 15°$, the number of calories disengaged per second is

$$\mathrm{K} = \frac{y\,\mathrm{I}^2}{4{,}154} = \frac{0\cdot 0747\,\theta^2 + 0\cdot 000024\,\theta^3}{104{,}000 + 434\,\theta - 0\cdot 397\,\theta^2 + 0\cdot 000083\,\theta^3}.$$

The cooling surface for a cylinder of 0·016 mètre diameter, and 0·400 mètre length, being approximately 20,000 square millimètres, $\dfrac{\mathrm{K}}{20{,}000}$ gives the fraction of a calorie disengaged per square millimètre of surface for each value of θ.

B. ABDANK ABAKANOWICZ—A NEW METHOD OF WINDING ARMATURES IN DYNAMO-ELECTRIC MACHINES.

(*La Lumière Electrique, T.* 13, *p.* 41, 1884.)

The author points out that the space occupied by wire on the armature of a dynamo is not filled with the copper conductor, when the section of the wire is circular, and that the empty spaces due to the round form of the wire amount to 12·5 per cent. of the total space. The author reviews the methods of winding with square wire and with ribands, adopted by Thomson and by Ferranti, and the suggestion of Mordey to fill the spaces left by round wire with a finer bare wire. He proposes to fill the bobbins with a flat wire or band, obtained in a peculiar way, by bending a strip of thin copper backwards and forwards, as a screen is folded on itself, then piercing the block thus formed, for inserting the core. The leaves are insulated from each other by bituminised paper. Each metal leaf, after being pierced, is cut through from outer to inner periphery with scissors, the cut being at top or bottom of each leaf alternately. The useless parts of the paper insulation are then removed, and the core inserted.

W. S. SCHULZ—ELECTRIC TRANSMISSION OF POWER IN MINES.

(*Zeitschrift des Vereines deutscher Ingenieure, p.* 149, 1884.)

The author deduces, from the executed electrical transmissions of power

tabulated below, that the maximum electrical efficiency of the receiving dynamo practically attainable is 50 per cent. of the transmitting motor:—

Name of Pit.	Power of Motor.	Machine Driven.	Length of Lead.	Efficiency.		
				Electrical	Mechanical.	Total.
	H.P.		Mètres.	Per cent.	Per cent.	Per cent.
La Peronnière, near Rive-de-Gier	37·1	Hauling-Machine	1,500	50·0	37·0	30
Thibaut, near St. Étienne	5·0	„	250	45·0	38·0	25
Blanzy	17·0	„	634	—	51·0 ?	30
Zaukeroda	14·7	Locomotive	893	46·6 ?	37·8 ?	30
„	5·0	Fan	780	50·0	26·0 ?	—
St. Claude, near Blanzy	8–10	„	900	—	—	—

On this basis he compares the cost of electrical transmission of power with the actual cost of transmission by compressed air and water applied to rock-drills and coal-getting machines, locomotives, cable and rope hauling-machines, and ventilators. He finds the former more costly than other methods, except where air is compressed by steam-power without being cooled, and in the case of locomotives for haulage on levels. The comparative cost of such haulage in actual operation is given in the table:—

UNDERGROUND HAULAGE BY LOCOMOTIVES.

—	Steam.		Compressed Air.		Electricity
	Dornan Pit.	Cesson Pit.	Petan's System.	Mékarski's System.	Zaukeroda Pit.
Cost of plant...	Marks. 32,000	Marks. 32,000	Marks. 13,000	Marks. 20,000	Marks. 16,000
Interest, etc., per ton-kilomètre	d. 0·264	d. 0·091	d. 0·490	d. 0·502	d. 0·276
Cost of haulage „ „	0·372	0·316	0·736	0·841	0·680
Total cost „ „	0·636	0·407	1·226	1·343	0·956
Daily work	Ton.-kms. 488	Ton.-kms. 1,421	Ton.-kms. 106	Ton.-kms. 159	Ton.-kms. 235
Length of line	Mètres. 2,320	Mètres. 4,627	Mètres. 620	Mètres. 620	Mètres. 620
Speed „	2·3	3·3	1·5	1·5	2·6
Weight of engine	Kilogrms. 4,400	Kilogrms. 8,000	Kilogrms. 2,700	Kilogrms. 2,300	Kilogrms. 1,600
Height „	Mètres. 1·92	Mètres. 2·1	Mètres. —	Mètres. 1·55	Mètres. 1·5
Width „	1·30	1·6	—	1·10	0·8

For the separate ventilation of a lateral drift of the Zaukeroda mine* a 40-inch Schiele's fan is used, driven by a Siemens dynamo actuated by a Dolgorouki steam-engine; the cost per 1,000 cubic mètres is 0·27*d.*, which is cheaper than transmission by compressed air only, where a special compressing plant for the small fan has to be put down. The author concludes that there is no field for electrical transmission in mining, special cases excepted, except for hauling on levels by locomotives, which could advantageously replace rope- or cable-haulage.

A. E. BOSTWICK—THE INFLUENCE OF LIGHT ON THE ELECTRICAL RESISTANCE OF METALS.

(*The American Journal of Science, Vol.* 28, *p.* 133, *August*, 1884.)

The influence of light on the electrical resistance of selenium has been investigated by W. G. Adams, Werner Siemens, and Sir William Siemens. It has also been shown that tellurium possesses the same qualities in this respect as selenium, though in a much less degree. Adams found the greatest diminution of resistance in tellurium to be 0·33 per cent. In 1877 Börnstein published a paper, in which he attempted to show that other metals, such as gold, silver, and platinum, possessed similar properties; but subsequently Werner Siemens and Hansemann concluded, from an elaborate series of experiments, that there was no action in the case of these latter metals. In 1881 Börnstein published a new series of experiments, from which it would appear that the electrical resistance of silver was diminished 0·0125 per cent. by the action of light.

Börnstein's first experiments were made upon platinum wires and thin leaves of gold, silver, and platinum, mounted on glass. Two methods were employed. The first consisted in the comparison of the resistance of two wires or plates, alternately illuminated. The second consisted in the observation of the logarithmic decrement of the swing of a galvanometer needle, the galvanometer being coupled in circuit with a single plate, which was alternately illuminated and darkened. The results obtained by the two methods varied very considerably. The later experiments of Börnstein were made entirely upon silver chemically deposited on glass, and consisted in the direct measurement of the ratio of the resistances of two plates by means of a Wheatstone bridge, with sliding contact-wire. The plates were exposed to the light for a period of fifteen minutes.

Werner Siemens and Hansemann in their experiments made no direct measurements, but noted merely the slight changes of position of the index of a sensitive galvanometer, coupled either in a Wheatstone bridge, or in circuit with a source of electricity and the metal plate to be examined, the plate being illuminated and darkened at short intervals.

In the author's experiments, carried out in the laboratory of Yale College, five plates, A, B, C, D, E, were employed. The plates A and B were of silver, deposited on plate glass, in a film thick enough to transmit about 50 per cent.

---

* *Minutes of Proceedings* Inst. C.E., vol. lxxiii., p. 474.

of light; C, D, and E were of platinum, gold, and silver respectively, deposited on glass in vacuo, and also each thin enough to transmit light easily. The observations were made to determine, first, the instantaneous effect of light, and secondly, the effect of exposure for from ten to fifteen minutes. Two methods of experiments were used, viz., the Wheatstone bridge method, in which one arm was formed by a single plate, and a similar method in which two arms of the bridge were formed by the plates A and B, which were then exposed to light alternately. The results are summed up in the following table:—

INSTANTANEOUS EFFECT. (METHOD I.)

Plate A.	Increase of a few thousandths of	1·00	per cent.
" B.	No diminution of more than ...	0·001	"
" B.	(Sunlight). Increase of	0·004	"
" C.	Increase of	0·0002	"
" D.	Increase of	0·0003	"
" E.	Increase of	0·00005	"

LONG EXPOSURES. (METHOD II.)

A and B. Mean effect on plate illuminated—Decrease of 0·0014 per cent.
A′ " B′. " " " Increase of 0·0053 "

Mean effect of Illuminating the plate.	Mean effect of Darkening the plate.
C. Increase of 0·004 per cent.	Increase of 0·0046 per cent.
D. Decrease of 0·003 "	Decrease of 0·014 "
E. Increase of 0·005 "	Increase of 0·007 "

These results are more in agreement with those of Siemens and Hansemann than with those of Börnstein, and show that if light causes any diminution in the electrical resistance of the metals experimented on, it probably does not exceed a few thousandths of 1 per cent.

MASCART—DETERMINATION OF THE OHM BY THE DAMPING METHOD.

(*Comptes Rendus, Vol.* 100, *p.* 309, 1885.)

With some few exceptions, the various methods for the determination of the ohm which were brought before the Paris Conference all gave values of the ohm greater than 1·06 mètres of mercury 1 square millimètre in section, except the damping method, which has always led to lower values; and the author has been led to investigate more closely the theory of this method, in order to see if this difference may be accounted for.

Without following the mathematical reasoning with which the paper opens, it may be said that Mascart arrives at a formula which is that given by Maxwell, viz.,

$$R = \frac{M^2 G^2}{2 K (\epsilon - \epsilon_0)} \left(1 + \frac{2 L}{R} \epsilon \right)$$

where R is the resistance of the coil, M the moment of the magnet, G the mean

galvanometer constant, K the moment of inertia of the magnet, L the coefficient of self-induction.

When the damping is considerable, the duration of the oscillations cannot be determined directly by experiment, and it is more general to deduce it from the relative duration, when the coil is on open circuit, by the formula

$$\epsilon^2 + \gamma^2 = \epsilon_0^2 + \gamma_0^2 = n_0^2.$$

But this formula it not strictly correct; in fact, the correction which ought to be introduced into the calculation of the resistance, in order to take into account the coefficient of self-induction, is half that which is usually taken by adopting the above formula.

This correction is, however, not the only one which has to be taken into account, but there is a more important one. In actual experiment the induced currents are necessarily very powerful, in order that the damping may be appreciable. These induced currents produce a temporary magnetism in the bar in a transverse direction, which must be taken into account. The intensity of the temporary magnetism of the bar is proportional to the action of the current. This magnetism does not modify the fundamental equation so far as the induction is concerned, but the earth's action on the bar introduces a new couple of opposite sign to that due to the permanent magnetism, and the value of this new couple must be subtracted.

The original hypothesis as to the smallness of the deflections and the constance of the galvanometer coefficient, G, may still leave some doubt as to the degree of exactitude of the result; but at any rate the two corrections referred to both tend to increase the values of the unit of resistance hitherto arrived at by the method of damping.

E. HOSPITALIER.—UNITY OF DEFINITIONS, NOTATION, AND SYMBOLS USED IN ELECTRICITY.

(L'Electricien, No. 89, 1884, and Bulletin de la Société Internationale des Electriciens, Vol. I., p. 366.)

The article refers to the determination of the above-mentioned society to appoint a committee to come to some conclusion on the advisability of introducing a stricter terminology in describing electrical phenomena. A few of the examples given by the author, of confusion in the use of terms, may be cited, with his suggestions for improvement.

The names "austral" and "boreal," as applied to the poles of a magnet, should be abolished, and that pole which points to the geographical north should be called the north pole of the magnet.

The confusion existing between the positive and negative pole of a battery, positive and negative electrode, and in the way of describing the grouping of the cells of a battery, should be cleared up.

A clear distinction should be drawn between the terms "generator," "receiver," and "transformer." The same remark applies to the terms "self-exciting" and "unipolar."

Coming to the system of C.G.S. units, the author points out the confusion which exists between the practical unit of power, the watt or volt-ampère, and the practical unit of work or energy, the volt-coulomb. It is also most desirable that uniformity should be introduced into the symbols used to represent the five chief electrical quantities, and that it should be no longer possible to find in the same book the same quantities represented by different symbols in different pages, or, what is worse, the same symbol used for different quantities.

As a result of M. Hospitalier's communication, an influential committee was appointed to codify the terms and symbols in ordinary use.

G. GALLICE—THE TRANSFERENCE OF CARBON IN THE ELECTRIC ARC.

(*L'Electricien, Vol. 9, No. 93, p.* 65, 1885.)

The following phenomenon may be observed by using a carbon 4 mm. in diameter as the positive, and one 15 mm. in diameter as the negative. The negative carbon, on account of its mass, is not rendered red hot. On the other hand, the positive carbon becomes at once incandescent, and wears to a point. Little by little, the particles of carbon which part from the positive are deposited on the negative, in the shape of a small cone. The cone has about 2 to 3 mm. diameter at its base, and attains a height of about 4 mm., when its formation ceases; the quantity which is burnt being exactly compensated by the quantity brought over from the negative electrode. If the cone is broken off, it will be formed again until the same height of 4 mm. has been reached. It is composed of an intensely black carbon, which has a sharply-defined and bright fracture, but is less coherent than the ordinary carbons.

A. BOUCHER—ELECTRICAL TRANSMISSION OF POWER AT BIENNE IN SWITZERLAND.

(*La Lumière Electrique, Vol.* 14, *p.* 216, 1884.)

There are two turbines, one working at high pressure and the other at low pressure, placed at the village of Boujeon, at the falls of the river Suze. The high-pressure turbine has been in use for rather more than a year, to work a dynamo machine for the electrical transmission of power to two factories at Bienne.

The total height of the fall is 177 feet, and the quantity of water 330 gallons per second; the turbine, however, only utilises actually 157½ feet of the fall, and can take, when the guide blades are wide open, 88 gallons per second. The turbine is one of Girard's, with a horizontal axle, driving a countershaft by spur gearing. The generator is a Thury dynamo machine of the type H_2, compound-wound, and is driven by a belt from the countershaft; it runs at a speed of 500 revolutions per minute, and gives an E.M.F. of 350 volts. The conductors consist of two copper wires 7 mm. in diameter (about

No. 2 B.W.G.), carried on telegraph posts and insulators. The motor is also a Thury machine of the H_2 type, compound-wound; it runs at a speed of 400 revolutions per minute, with a difference of potential of about 300 volts at the terminals. The distance from the generator to the motor is about 1,367 yards, and the motor gives out from 6 to 18 horse-power, according to the exigencies of the work being done in the factories. Both ends of the line are protected by lightning-conductors, a very necessary precaution in a district where thunder-storms are very prevalent. When starting the motor, a resistance is at first switched into the circuit, but this is cut out step by step as the machine starts. The author promises exact figures of the working, but states that a commercial efficiency of 70 per cent. is obtained.

B. MARINOVITCH—HEATING OF CONDUCTORS.

(*La Lumière Electrique, Vol.* 14, *p.* 252, 1884.)

This article is itself an abstract of one by Mr. A. Perenyi, which appeared in the August number of the *Elektrotechnische Zeitschrift* (Vol. 5, p. 321, 1884).

If we consider the case of a wire which has a length of l mètres, a diameter of d millimètres, and a specific resistance a (*i.e*, the resistance of 1 m. with an area of 1 mm.²), the energy spent in unit of time for a current of I ampères is

$$Q = \frac{4\,a\,l\,I^2}{\pi \times 9{\cdot}81 \times 424 \times d^2} = \frac{4\,a}{4160\,\pi}\,l\,\frac{I^2}{d^2} \quad \ldots \quad \ldots \quad \ldots \quad (1)$$

The quantity of heat required to produce an excess of temperature T at the end of a time t will be

$$U\,t = \omega\,\frac{\pi\,d^2\,l}{4}\,T = A\,T \quad \ldots \quad \ldots \quad \ldots \quad (2)$$

where ω is the specific heat of the metal.

If V is the rate of cooling, then

$$Q\,dt = U\,dt + V\,dt.$$

But

$$U\,dt = A\,dt,$$

$$\therefore Q\,dt - V\,dt = A\,dt,$$

or

$$t = A\int_0^T \frac{d\,T}{Q - V} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (3)$$

Q is a constant, but V is a function of T of the form $V = B\,T$, where B is the loss of heat in unit of time for an excess of temperature of 1 degree. Integrating, we obtain

$$T = \frac{Q}{B}\left(1 - \epsilon^{-\frac{B}{A}\cdot t}\right) \quad \ldots \quad \ldots \quad \ldots \quad (4)$$

In practice, the important case is that where the energy developed Q does not raise the temperature of the body, but is transmitted to the surrounding media; *i.e.*, when $U = 0$ and $Q = V$.

From the formulæ of Dulong and Petit, taken in conjunction with those of Peclet, we find the heat lost by a conductor in one hour is

$$3{,}600\,V = (S\,K + S'\,K')\,10^{-3}\,d\,l\,\pi \quad \ldots \quad \ldots \quad (7)$$

where S *and S' represent functions of* the excess of temperature for heat lost

by radiation and by convection, K is a coefficient of radiation which varies with the nature of the conductor, while K′ is a coefficient of convection which depends on the shape and position of the conductor. For horizontal cylindrical surfaces, according to Peclet, $K' = 2{\cdot}058 + \frac{76{\cdot}4}{d}$ per hour per square mètre, and for vertical cylindrical surfaces $K' = \left(0{\cdot}726 + \frac{1{\cdot}543}{\sqrt{d}}\right)\ \left(2{\cdot}43 + \frac{0{\cdot}8757}{\sqrt{l}}\right)$.

The energy dissipated per hour for a constant current I is, from equation (1), for copper

$$3{,}600\ Q = 2{\cdot}2984 \times 10^{-2}\ \frac{I^2}{d^2}\ l \quad \ldots \quad \ldots \quad (8)$$

and by equating formulæ (7) and (8), *i.e.*, by making $U = 0$,

$$7{\cdot}324\ \frac{I^2}{d^3} = 0{\cdot}16\ S + S'\ K' \quad \ldots \quad \ldots \quad \ldots \quad (9)$$

The values calculated from this formula have been compared with those found experimentally by Professor Forbes, and agree very closely.

Taking the tables of Peclet, it is seen that for copper K is very small in comparison with K′, hence equation (9) may be written

$$7{\cdot}324\ \frac{I^2}{d^3} = S'\ K' \quad \ldots \quad \ldots \quad \ldots \quad (10)$$

in which $S' = n\ T^b$.

Finally, replacing K′ by its value as given in the formula for horizontal cylindrical surfaces, and taking Dulong's values, $n = 0{\cdot}3577$, $b = 1{\cdot}233$,

we have

$$\frac{I^2}{d^3} = \frac{0{\cdot}\overset{3577}{\cancel{8597}}}{7{\cdot}324}\left(2{\cdot}058 + \frac{76{\cdot}4}{d}\right) T^{1{\cdot}233} \quad \ldots \quad \ldots \quad \ldots \quad (11)$$

which may be written

$$\left(\frac{I}{d}\right)^2 = \frac{0{\cdot}\overset{3577}{\cancel{3557}} \times 2{\cdot}058}{7{\cdot}324}\left(d + \frac{76{\cdot}4}{2{\cdot}058}\right) T^{1{\cdot}233} \quad \ldots \quad \ldots \quad (12)$$

whence

$$T^{1{\cdot}233} = \frac{9{\cdot}977}{d + 37{\cdot}12}\left(\frac{I}{d}\right)^2 \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (13)$$

If now we neglect d in comparison with 37·12, or 37·12 in comparison with d, we have either $\frac{I}{d}$ a constant or $\frac{I^2}{d^3}$ a constant, neither, however, being rigorously exact.

Mr. Perenyi has also investigated the question of the temperature at the centre of the conductor as a function of that at the surface; and, without following the calculation, it may be said that the difference of temperature between the centre and the surface of the conductor is very approximately directly proportional to the square of the current, and inversely proportional to the square of the diameter.

G. LIPPMANN—ACTION OF HEAT ON BATTERIES, AND THE LAW OF KOPP AND WOESTYNE.

(*La Lumière Electrique, Vol.* 14, *p.* 388, 1884.)

It is known that some batteries have an E.M.F. which varies with their *temperature; such* are especially those which have a solid depolarising element.

It is also known that batteries with a variable E.M.F. have a very peculiar property, namely, that the energy which they produce under the form of an electric current is not equal to the energy of the chemical action going on, as measured by a calorimeter: the difference between the two quantities is due to the fact that heat drawn from the surrounding media is joined to the heat due to the chemical action, and the two are together transformed into electrical or mechanical work. This borrowing of heat from the surrounding media has been shown analytically by Helmholtz, and verified experimentally by Czapski.

From a consideration of six differential equations, which are arrived at by an application of the laws of thermo-dynamics, the author concludes that batteries with constant E.M.F. are those which satisfy the law of Kopp and Woestyne.

When in a series of chemical reactions there is a change of state,—for example, the solution of a solid body,—the law of Kopp and Woestyne is not verified, as Mr. Berthelot showed long ago. It is for this reason that batteries with a solid depolarising element are sensible to changes of temperature. Moreover, if in a Clark's cell containing a saturated solution of zinc sulphate, an excess of crystals of this salt are added, the increase of the E.M.F. for one degree is raised by one-third of its value.

Mr. Berthelot has also shown that the reactions in which the chemical heat varies with the temperature are those where the law of the calorific capacities are not verified. This proposition is not to be confounded with the one stated above, since the E.M.F. is not always proportional to the chemical heats. Uniting the two propositions, it may be said that, if the law of calorific capacities is verified, the chemical heat and the E.M.F. are equal, and are unaffected by changes of temperature; in the opposite cases, the two quantities are unequal, and are variable with the temperature.

E. WARBURG—ELECTROLYSIS OF SOLID GLASS.

(*La Lumière Electrique, Vol.* 14, *p.* 390, 1884.)

Buff had observed that at temperatures of about 300° C. the current traversing the glass at first gave a considerable deflection on a galvanometer, which subsequently fell off and ultimately became nearly nil. He imagined that this was due to polarisation. Mr. Warburg has repeated the experiments, and has arrived at the conclusion that the effect is due to the formation of an insulating layer of silicon on the surface of the anode, resulting from the electrolysis of the silicate of soda in the glass.

The experiments were carried on in a hot-air stove, heated by Bunsen burners, and kept at a constant temperature of about 300° C. by a regulator. The test-tubes used were of Thuringian glass, which is relatively a good conductor, and the two surfaces of which were in contact with mercury. If the above hypothesis is true, the siliceous layer forms the insulating layer of a condenser, the conducting plates of which are, on the one side, the mercury, and *on the other the glass.*

From the sum of his experiments Mr. Warburg has derived the value 0·0221 microfarads per square centimètre for the capacity of the siliceous layer, which in this case had been deposited by the current from 30 Bunsen cells. From the capacity, the thickness of the layer can be calculated, and was found to be $4{\cdot}71 \times 10^{-5}$ millimètres.

The presence of this siliceous layer prevents the deposition of any film of moisture, and it therefore becomes of practical use in assuring the perfect insulation of glass tubes. The layer is very adherent, and can only be removed by prolonged soaking in strong caustic potash.

By using as the anode an amalgam of sodium (½ to 1 per cent.), the siliceous layer is no longer formed, and the passage of the current does not in any way change the constitution of the glass. The glass takes up from the amalgam a quantity of sodium equal to that which it gives up to the mercury kathode. By putting the amalgam outside the tube, and then weighing the tube and the contained mercury after electrolysis, it was found that the weight had increased by a quantity proportional to that of the silver deposited in a voltameter in the same circuit. It appears, therefore, that the silicate of sodium is the electrolyte, and that the sodium is carried forward with the current.

P. BAKMETIEFF—THE RELATION BETWEEN THE MAGNETIC MOMENT OF A BUNDLE OF IRON WIRES, ITS MASS, AND THE DIAMETER OF THE WIRES.

(*La Lumière Electrique, Vol.* 14, *p.* 391, 1884.)

The magnetic moment of a bundle has often been compared with that of a solid cylinder, but the influence of the diameter of the wires has not been studied. The author filled the hollow interior of a cylindrical bobbin, 148 mm. long and 32·2 mm. interior diameter, with iron wires having respectively diameters of 1 mm., 2 mm., and 5 mm., and magnetised them by currents of various strengths. The magnetic moment of the bundle of the finest wires, as well as its specific magnetism, exceeded those of the two others, and was especially remarkable for strong currents. When, instead of filling up the whole hollow of the bobbin, only a circular ring of wires was used, the specific magnetism of the thin wires became for the same current strength several times greater than that of the thicker wires.

Dr. S. L. ANGELINI—VARIATION IN RESISTANCE OF A GERMAN SILVER WIRE WHEN STRETCHED.

(*La Lumière Electrique, Vol.* 15, *p.* 41, 1885.)

After a reference to the labours of Mousson, De Marchi, and Tomlinson, the author goes on to describe some experiments made in the laboratory of the Padua University, under the direction of Professor Rossetti.

The wire experimented upon was 1·2 mètres long, with a diameter of 0·355 *mm. (No. 28 B.W.G.)*, and had an initial resistance of 2·243312 ohms at 21° C.

It was suspended vertically, and was stretched by weights placed in a scale-pan at the lower extremity; an additional weight of 400 grammes was added every two minutes until the wire broke with a total load of 1·06 kilogrammes (say, 2 lbs. 5½ oz.). The lengthening of the wire was carefully measured by a cathetometer reading to $\frac{1}{25}$ of a millimètre; and a plumb-line suspended alongside served to indicate any give in the support from which the wire was hung.

The measurements of resistance were made by means of a Wheatstone bridge, in one arm of which was connected up the German silver wire, and in the other a Poggendorf rheostat, and an additional rheostat formed by a thick German silver wire, each millimètre of which had a resistance of 0·00021 ohm. A galvanometer by Edelman was used, which was rendered very sensitive by means of a powerful directing magnet. The deflections were read on a scale by a telescope, and the galvanometer being short-circuited between each approximation to the true resistance, the zero was re-established for each reading. The current was furnished by six small Leclanché cells, and was kept constantly circulating in the bridge and wire under observation; the quantity of heat produced, which was very trifling, therefore remained constant. The slight delay caused by making the successive approximations served to allow the wire to regain the heat lost by stretching.

The results are given in a table, from which a few observations are chosen as examples:—

Weight. Grammes.	Resistance measured.	Resistance calculated.	Resistance measured. Corrected by interpolation.	Resistance calculated. Corrected by interpolation.	Differences.
3,000	2·25073	2·24931	2·25073	2·24918	0·00155
5,000	2·25965	2·25906	2·25879	2·25827	0·00052
6,200	2·26394	2·26237	2·26420	2·26266	0·00154
8,200	2·27433	2·27015	2·27485	2·27063	0·00422
9,800	2·28695	2·27977	—	—	—
10,600	2·29271	2·28471	—	—	—

By constructing a curve for the variation of the resistance between the loads 1,800 grammes and 9,000 grammes, taking the loads as abcissæ and the resistances as ordinates, it can be seen that—

1. The resistance increases with the load.

2. Between 1,800 and 3,400 grammes the increase of resistance is almost proportional to the increase of load. After this point the increase of resistance is more rapid, though still proportional to the load up to 7,800 grammes, when again the curve becomes steeper. There are then two critical points, one at 3,400 grammes, the other at 7,800 grammes—points at which a change in the molecular structure of the wire occurs.

After the load had been *taken off*, by fracture of the wire, the resistance

went back at once to 2·24757 ohms, corresponding to a load of 1,570 grammes, which resistance very gradually diminished.

In another column of the table the author has given the differences between the observed and the calculated values, and, forming a curve with these, it will be seen that—

1. The observed resistance is always greater than that calculated from the formula $\left(R^1 = R\frac{l_1^2}{l}\right)$, where R and l are the initial resistance and length, and l_1 the length after stretching.

2. The curve shows two points of change of curvature, one at 3,000 grammes and the other at 7,400 grammes.

3. The portion of the curve between these two values has the form of a parabola. From the 7,000 grammes till the breaking point, the curve shows sudden variations due to molcular changes.

For the parabolic portion of the curve, the author has found the equation

$$D = 0\cdot002273 - 0\cdot000682\ T + 0\cdot000066\ T^2,$$

where D is the difference in resistance and T is the load: from this equation the mean error comes out $\pm$ 0·00014, which indicates that the equation may be relied upon.

F. KOHLRAUSCH—THE CONDUCTION THEORY OF THERMO-ELECTRICITY.

(*Annalen der Physik und Chemie, Vol.* 23, *H.* 3, *No.* 11, 1884, *p.* 477.)

The article is a reply to Budde's objection to the author's conduction theory, according to which the thermo-electro-motive force is to be sought in the heat currents which exist in every actual thermopile; Budde being a supporter of the contact theory, which recognises the seat of the thermo-electric force as being at the junctions.

Budde objects that the conduction theory can only agree with the second law of thermo-dynamics if the thermo-electric series can be represented by a quadratic expression as a function of the temperature. In order to explain the Peltier effect, the author has assumed that the electric current carried heat along with it; and to this Budde strongly objects, since he says there can be no sense in saying that heat, which is not material, but only a form of energy, is capable of motion in a wire at a constant temperature. Differences of the heat distribution may be equalised, but where there are no differences there cannot be anything of the nature of a current.

F. KOHLRAUSCH—CONDUCTIVITY OF WATER DISTILLED IN A VACUUM.

the a (*Annalen der Physik und Chemie, Vol.* 24, *Pt.* 1, *p.* 48, 1885.)

Padua apparatus used by the author in his experiments was of the nature of

The hammer, and consisted of two glass bulbs, one much larger than the

mm. (No. 2

other, connected by a glass tube provided with a short side tube, by means of which the apparatus could be connected to a Sprengel pump. In the smaller bulb were two platinum electrodes, by means of which to measure the resistance. The whole apparatus was filled through the side tube with the purest water obtainable, and then boiled, the pump being worked until the pressure was about one-hundreth of a millimètre of mercury, the vapour at the end being absorbed by cooled sulphuric acid. The side tube was then hermetically sealed by a blow-pipe. The water left in the apparatus filled about two-thirds of the larger bulb, and by warming this bulb to 40°, while the smaller one was kept at 0° to − 8°, some 6 to 8 cubic centimètres of water could be distilled over.

The resistance was measured on a Wheatstone bridge, very short intermittent currents being sent through it from two Smee cells; a very sensitive mirror galvanometer being employed to get the balance. The current was so weak that polarisation did not occur for about one-tenth of a second, which was a longer time than the current was kept on.

The resistance of the water did not seem to be constant in all cases, as it was observed to diminish with lapse of time, the conductivity in one tube having increased some twenty-five times in fifteen hours.

The figure which the author considers from a review of all his experiments to represent the conductivity at 18° C. of water distilled in vacuo, taking mercury as unity, is 0·000000000025. Put in other words, this means that a conductor of mercury encircling the earth would have the same resistance as a conductor of water *one millimètre* long, the sections being equal. The ohm would be represented by a layer of water one square millimètre in area and some twenty-six billionths of a mètre thick, or, again, a column of water one square millimètre in section and one mètre long wonld have a resistance of 4×10^{10} ohms; a copper wire one square millimtère in section, to be of the same resistance, would have a length of 24×10^{8} kilomètres, a distance which it would take a ray of light some two and a quarter hours to traverse.

F. RICHARZ—FORMATION OF OZONE, PEROXIDE OF HYDROGEN, AND SULPHUR TRIOXIDE IN THE ELECTROLYSIS OF DILUTE SULPHURIC ACID.

(*Annalen der Physik und Chemie, Vol.* 24, *p.* 183, 1885.)

In his experiments the author at first made use of a U tube, but this had to be abandoned, as the results were not concordant, owing to the concentration of the sulphuric acid in the leg of the tube containing the positive electrode. A straight tube was then used, which was clamped in a vertical position. The positive electrode was formed by a platinum wire placed in the upper part of the tube, the two ends of the wire being sealed into the glass at two points vertically below each other. The object of having both ends brought out of the tube was in order to enable the wire to be brought to a red heat by the passage of an electric current, it having been found that, unless this were done previously to each experiment, the results did not agree. The negative

electrode finally decided upon, after a trial of thin plates of platinum, was a similar platinum wire fused into the opposite side of the tube, and lower down than the positive one. The whole apparatus was kept at zero temperature by being surrounded with melting ice; and the current used for effecting the electrolysis was kept constant by observing the deflections of a tangent galvanometer, and putting in or cutting out portions of a resistance which formed part of the circuit. The galvanometer constant having been accurately determined, it was also possible to calculate the total amount of gases which should have been liberated by the passage of the current, and thus to check the quantities determined by the chemical analysis.

The quantity of ozone formed was determined by the amount of iodine set free from a solution of potassium iodide, this amount of iodine being estimated by its reaction on normal sodium sulphite. The sulphur trioxide formed was calculated from its action on ferrous sulphate, which it converts into ferric sulphate. For the determination of the hydrogen dioxide, the permanganate of potash test was used.

At first the positive platinum wire electrode was not heated, and it was then found that the results did not agree. The author sought for the cause of this, in the first instance, in the negative electrode, but he found that this did not affect the result, nor did the evolution of hydrogen, nor impurities such as lead or manganese in the electrolyte, and he therefore concludes that this alteration of the positive electrode was due to the evolution of oxygen.

The reduction of the sulphur trioxide at the negative electrode causes a disappearance of a quantity of hydrogen equivalent to its own oxygen, which will be shown by a deficiency in the total quantity of gases produced. It was clearly proved that the sulphur trioxide was reduced by the nascent hydrogen; also that the formation of these higher oxides is dependent on the temperature, and that the quantity of sulphur trioxide decreases much more quickly with rise of temperature than the ozone. The author also found that the quantity of these evolved gases increased with the current density, and that he consequently got better results when using wires than when using plates. The degree of dilution of the sulphuric acid also has its influence on the amount of gas evolved, and it was found that the largest quantity of sulphur trioxide was formed when the electrolyte contained from thirty to forty per cent. of sulphuric acid. It was more difficult to determine the quantity of ozone, but it seemed at any rate that with a solution containing more than sixty per cent. of acid the quantity was less than with more dilute solutions.

VELLONI—CELL WITH CONSTANT CURRENT.

(*Beiblätter, Vol.* 9, *p.* 46, 1885; *Zeitschrift für Instrumentenkunde, Vol.* 4, *p.* 402, 1884.)

This is a form of bichromate cell, the chief improvement being in the exciting liquid. Ten litres of a saturated solution of bichromate of potash at 80° C. are mixed with 70 cubic centimètres of nitric acid, and the crystals *which separate out* at 40° C. are mixed with sulphuric acid.

G. VINCENTI—CONDUCTIVITY OF ALCOHOLIC SOLUTIONS OF SOME CHLORIDES AND OF VERY DILUTE SALINE SOLUTIONS.

(*Beiblätter*, *Vol.* 9, *p.* 131, 1885.)

The experiments were made on Kohlrausch's plan, with alternating current furnished by an induction-coil, the resistances being measured on a Wheatstone bridge by means of an electro-dynamometer of Bellati's construction.

The coefficients of conductivity are calculated from the formula

$$k = \lambda m - \lambda^1 m^2,$$

where $m = 10 \, . \, ps/A$; p is the percentage of the solution, s its specific gravity, A the molecular weight of the dissolved substance.

Hence we have the following values:—

SUBSTANCE.	IN ALCOHOL.		IN WATER.
	$\lambda \times 10^8$	$\lambda^1 \times 10^8$	$\lambda \times 10^8$
Ammonium Chloride...	170·2	484	954
Lithium "	167·5	530	701
Magnesium "	67·3	238	719
Calcium "	47·2	124	750
Copper "	15·0	20	—
Cadmium "	11·3	18	365
Zinc "	6·4	?	681

The resistances of the alcoholic solutions are therefore much greater, and the sequence of the series is not the same. With dilute solutions the conductivity increases less quickly than the degree of concentration. The temperature coefficients are somewhat smaller than in water solutions, and approach fixed limits. Thus, for temperatures between 18° and 25°, we have—

	m	$\frac{10^4 \, \delta \, k}{k_{18}}$
Ammonium Chloride...	0·089 — 0·025	146 — 162
Lithium "	2·894 — 0·041	263 — 165
Magnesium "	0·150 — 0·050	108 — 105
Calcium "	1·034 — 0·048	141 — 122
Copper "	6·654 — 0·110	321 — 105
Cadmium "	0·145 — 0·075	171 — 191
Zinc "	C·913 — 0·252	191 — 194

Using the same method of observation for very dilute solutions, the author concludes that the conductivity increases more slowly than the degree of

concentration; that the different salts, when equally and very largely diluted, have not the same molecular coefficient of conductivity; that the influence of temperature is nearly the same for different salts and increases with it, so that the conductivity c_t for the temperature t is $c_t = c_0 (1 + a\,t + b\,t^2)$, where a very closely corresponds to the change of the coefficient of friction of water with temperature.

EDELMANN'S VARIABLE AMMETER SHUNT.

(*Centralblatt für Elektrotechnik, Vol.* 6, *p.* 670, 1884.)

At the left-hand end of a stout wooden base are fixed two blocks of metal in line, and at the right-hand end are three similar blocks. Between these blocks are stretched four stout copper wires, or, if necessary, rods may be used: these wires are arranged in the metal blocks, so that wire No. 1 has one end in the top right-hand block, the other end and one end of wire No. 2 being in the top left-hand block; the middle right-hand block is the terminal for wires 2 and 3, the lower left-hand block is the terminal for wires 3 and 4, and the lower right-hand block is the terminal for the other end of wire 4. The leads from the dynamo machine are brought to the upper and lower right-hand blocks, so that the current must follow a zigzag course through all four wires. At seven equidistant points of the wires are soldered smaller pieces of wire, which serve as terminals for the galvanometer circuit; while six more terminals, equidistant among themselves, but at $\frac{1}{5}$ the distance of the former seven, serve for a similar purpose. In this way 35 different lengths of the shunt may be included between the terminals of the galvanometer, each length being $\frac{1}{35}$ of the total resistance of the shunt.

EDELMANN'S PORTABLE ASTATIC GALVANOMETER.

(*Centralblatt für Elektrotechnik, Vol.* 6, *p.* 775, 1884.)

The lower of the two needles hangs inside a copper damper which is placed inside the coil; the upper needle, to which is affixed an aluminium pointer, hangs above the graduated circle. The coil, together with the damper, etc., are enclosed in a wooden cylindrical box, to which the two terminal screws are fixed; while the graduated circle is contained in a glass-capped cylinder placed on the top of the wooden box. The whole is movable on a central pivot provided with a three-legged base. The needle is suspended by an eye to a fibre, which is fixed to a torsion head at the top of a vertical tube fastened on the glass plate by a screw cut at its end. By means of this screw the whole tube with suspension and magnets can be lowered, so that the tube rests on the upper magnet, which itself lies on the divided circle. In this way the needle is firmly gripped, and the instrument can be moved about without fear of breaking the suspension fibre. The divided circle is movable, so that it can be turned round to make the zero point coincide with the plane of windings of the coil.

Dr. H. KRÜSS—A NEW FORM OF BUNSEN PHOTOMETER.

(*Centralblatt für Elektrotechnik, Vol.* 6, *p.* 781, 1884.)

In the oldest form of Bunsen photometer, in which the two sides of the screen had to be compared together, an error was introduced from the fact that the light incident on the screen was not separated into two parts, the transmitted and the reflected, but that a portion of the light was absorbed. There are therefore three positions of the screen, viz., that in which the grease-spot is invisible on the left-hand side, that in which it is invisible on the right-hand side, and that in which it appears equally distinctly on both sides. In Desaga's form of photometer only that position was used in which the grease-spot disappeared on one side. Rüdorf introduced the arrangement in which the screen stands at the bisection of two mirrors which make an angle of about 140° with each other. In this arrangement, therefore, we compare the two images of the opposite sides of the grease-spot, which appear dark on a light ground. One of the chief objections to this arrangement is that the two images are somewhat widely separated, and it is therefore more difficult to make the comparison. This objection has been noticed by Von Hefner-Alteneck, who adopted the plan of placing two prisms in front of the screen, instead of two mirrors behind it. This improvement is, however, itself open to the objection that the images of the grease-spot are formed by refraction, in consequence of which the images are slightly distorted, and, what is far worse, they may be chromatic.

In the author's own arrangement, two prisms are used in front of the screen, but their faces are so cut and they are placed in such a position that the images of the two sides of the grease-spot are formed in close juxtaposition by rays which, falling normally on the first face of the prism, are unaltered in direction; they are then thrice totally reflected from the interior faces, and finally come out normal to the fifth face. The two prisms are in contact, and in front of the faces whence issue the rays is fixed a tube, closed at the farther end by an eye-piece, which ensures the observer's eye being exactly in the plane of the screen. The field of view is then exactly divided in the centre by the junction of the two prisms; and if the screen is correctly placed, the two images of the grease-spot will lie one on either side of the medial line in immediate contact, and can be most readily compared together.

Dr. L. WEBER—CURVES FOR THE CALCULATION OF THE ILLUMINATING POWER OF LIGHTS.

(*Elektrotechnische Zeitschrift, Vol.* 6, *No.* 2, *p.* 55, 1885.)

The question which we most want to solve with any illuminant is not so much the intensity of the source of light as the degree of illumination which it will give to surrounding objects. To facilitate the solution of this question, the author has constructed a diagram of curves, from a mere inspection of which it can be seen what will be the illumination given to any surface, horizontal or vertical, by a light of known power. The calculation by which

the author has worked out his diagram may be briefly resumed, and perhaps one concrete example may make the matter more clear.

Imagine a source of light placed at a height y above any horizontal plane, and consider any small element of the plane surface which lies at a distance x from the foot of the perpendicular let fall from the light; let r be the distance from the light to the element of the surface, *i.e.*, r is the hypothenuse of a right-angled triangle, of which x is the base and y the perpendicular; let the angle of incidence of the light, *i.e.*, the angle between r and the vertical, be i, and let the angle between r and x be a, so that $\sin. a = \cos. i$. Then if I is the intensity of the source of light, and Q is the quantity of light on the element of the surface f,

$$Q = \frac{I \cdot f \cdot \sin. a}{{}^2r},$$

or, if H is the illumination of the small element of surface,

$$H = \frac{I \,.\, \sin. a}{r^2} = \frac{I \,.\, y}{(x^2 + y^2)\sqrt{x^2 + y^2}}.$$

Example:—A glow lamp which gives a light of 32 candle-power in a horizontal direction was found to give 30·43, 27·2, 22·62, 25·92 under the angles 22·5°, 45°, 67·5°, 90° respectively, as measured below the horizontal line. Suppose this lamp placed at a height of 0·8 mètre above a table, what is the light at a point 0·67 mètre from the foot of the standard carrying the lamp? It will be found that r is equal to 1·044 mètres, and hence $\sin. a = \frac{0{\cdot}8}{1{\cdot}044} = 0{\cdot}7667$, or a = about 50°. At this angle the illuminating power of the glow lamp is found by interpolation to be about 26·2 candles. Hence $H = \frac{26{\cdot}2 \times 0{\cdot}7667}{1{\cdot}0889}$ = 18·45 candles, *i.e.*, the amount of light at the point of the table under consideration is the same as would be given by 18·45 standard candles at a distance of 1 mètre. The diagram of curves given with the paper is calculated on the assumption that the source of light has an intensity of 100 standard candles; and by reading the distances in mètres horizontally and vertically, it can be seen at a glance what number of candles at a distance of 1 mètre would have the same illuminating power.

W. PEUKERT—RESISTANCE OF THE ELECTRIC ARC.

(*Zeitschrift für Elektrotechnik, Vol.* 3, *p.* 111, 1885.)

The quantities measured were the strength of the current, the difference of potential between the two carbons, and the distance between their points. For measuring the current, one of Crompton and Kapp's ammeters was used, which had been previously calibrated; a Siemens torsion galvanometer was used for measuring the difference of potential between the carbons.

The length of the arc was determined as follows:—Behind the lamp was a vertical millimètre scale, the carbons were placed at about the desired distance apart, and then the circuit was completed by bringing a third carbon between the other two; as soon as the current and E.M.F. had become constant, the

current was interrupted, and the distance apart of the carbon points was read on the millimètre scale. In order to avoid any errors which might be introduced by the crater formed in the positive carbon,—the current was produced by a Siemens compound-wound direct-current machine,—the carbons were filed to a fresh point before each measurement.

The plotted values of the resistances and lengths of arc appear as straight lines, which do not pass through the origin, but start at some distance above it. It was found that the resistance could be expressed by a linear equation of the form $R = a + b L$.

For the several strengths of current used in the experiment, the constants a and b had the values shown in the following table:—

Current.	a	b
10 ampères	3·66	0·23
15 „	2·3	0·15
20 „	1·8	0 08
25 „	1·3	0·075
30 „	1·6	0·04

The fifth series of experiments do not permit of direct comparison with the other four, as the carbons had been changed.

The resistance of the arc therefore consists of two parts—of a constant quantity for the same current, and of a quantity which increases with the length of the arc.

If we consider that the resistance represented by the constant a is due to the passage of the electricity from the positive carbon to the air, and represent this for unit section by the symbol r, then

$$R = \frac{r}{Q} + S \cdot \frac{L}{Q},$$

where Q is the sectional area, and S the specific resistance of the carbons.

The numerical values found in the above table for $\frac{r}{Q}$ show that this resistance is inversely proportional to the strength of the current, and confirm Dr. O. Frölich's view that the sectional area of the arc increases in proportion with the current. If, then, we put $Q = c I$, we have

$$R = \frac{r}{c I} + \frac{S L}{c I},$$

so that the constant $b = \frac{S}{c I}$.

The equations formed with the values of a and b given in the above table show that b decreases more rapidly than the current increases. This was to be expected, since with stronger currents, in consequence of the increase of temperature, the conductivity of the carbon is increased, and the heated air itself conducts better.

On the supposition that a polarisation takes place in the arc, we have for

it the value a I, since R I = a I + b . I . L. If this value a I is calculated from the values of a and I in the foregoing table, we find a mean value of 35 volts. This value, which is so much higher than any which has before been found for polarisation, seems to prove conclusively that the constant part of the resistance of the arc cannot be set down to this cause. It seems, therefore, that a part only of the apparent resistance of the arc can be considered as a true resistance, and that the greater part is due to a mechanical resistance, in overcoming which a part of the energy of the current is used up.

LIST OF OTHER ARTICLES.

(Comptes Rendus, Vol. 99.)

No. 24.—**D. TOMMASI**—Electro-chemical Equivalents. **A. CORNU**—Shape of the Surface of a Wave of Light in a Magnetic Field.

No. 25.—**P. DUHEM**—Thermo-dynamic Potential. **A. CROVA**—Diffusion Photometer.

(Vol. 100.)

No. 1.—**P. DUHEM**—Theory of Electro-dynamic Induction.

No. 2.—**T. MOUREAUX**—Present Value of Magnetic Elements at St. Maur Observatory. **JEANNOLLE**—Use of Electricity for Prevention of Scale in Boilers. **HURION**—Changes in the Resistance of Bismuth when in a Magnetic Field.

P. 1067.—**A CROVA**—Photometry of Intense Lights.

(L'Electricien.)

No. 89.—**H. FONTAINE**—Copper Refining.

No. 90.—**L. GREZEL**—Battery with Insoluble Negative Electrode.

No. 91.—**E. HOSPITALIER**—Alternate-current Motors.

No. 95.—**E. HOSPITALIER**—Electro-chemical Equivalents.

(La Lumière Electrique, Vol. 14.)

No. 51.—**F. CARDARELLI**—New Method of determining the Resistance of a Fault in a Submarine Cable.

(Vol. 15.)

No. 3.—**A. PACINOTTI**—Artificial Magnetisation of Magnetite (Loadstone).

No. 5.—**G. MARESCHAL**—Lippmann's Electric Meter.

No. 7.—**Dr. KITTLER**—Measurement of Current Strength.

(Annalen, Vol. 23.)

No. 4.—**E. PFEIFFER**—Resistance of Water charged with CO_2. **O. BOEKMANN**—*Resistance* of Microphone Contacts during Vibration. **H. WILD**

—Determination of Value of Siemens' Unit in Absolute Magnetic Measure. **J. ROSENTHAL**—A New Galvanometer. **K. SCHERING**—Quadrifilar Magnetometer: a New Instrument for Measuring the Earth's Vertical Force.

(*Vol.* 24.)

No. 1.—**A. SCHUSTER**—Electric Discharge in Gases. **H. HERTZ**—Dimensions of the Magnetic Pole in Various Systems of Measurement.

No. 2.—**RAYLEIGH**—Damping Method for determining the Ohm.

(*Beiblätter, Vol.* 8.)

No. 12.—**G. GUGLIELMO**—Determination of E.M.F. and Resistance of Cells, and of Polarisation by Strong Currents. **A. ROITI**—Measurement of Capacity of a Condenser in C.G.S. Units. **W. VAN LOGHEM**—Theory of the Reflection of Light from Magnets.

(*Vol.* 9.)

No. 2.—**A. LEDUC**—Changes in Resistance of Bismuth with Temperature. **B. v. KOLENKO**—Pyro-Electricity of Quartz.

(*Centralblatt, Vol.* 7.)

No. 3.—**Dr. E. BÖTTCHER**—Zinc-Manganese Battery.

No. 4.—**PECHAN**—Winding of Dynamo Machines.

(*Elektrotechnische Zeitschrift, Vol.* 6.)

No. 2.—**Dr. W. GIESE**—Earth Currents at Kingawa observed by German Polar Expedition. **A. BERNSTEIN**—E.M.F. of Batteries.

No. 3.—**A. GRAVIER**—Influence of the Iron in a Gramme Ring on its Action. **OELSCHLÄGER**—Heating of Wires by Electric Current.

(*Zeitschrift für Elektrotechnik, Vol.* 3.)

No. 1.—**Dr. A. v. WALTENHOFEN**—Carrying Power of Magnets. **KRIZIK**—Attraction of Cores by Solenoids.

No. 2.—**KESSLER**—Direct Measurement of Current, E.M.F., and Resistance in Practical Units (Ampères, Volts, and Ohms) by Means of Tangent Galvanometer. **T. SCHWARTZE**—A New Method for Production of Electricity.

No. 3.—**KRIZIK**—See *ante*, No. 1. **KESSLER**—See *ante*, No. 2.

ERRATA.

Mr. F. M. Wright has pointed out to me an error in my paper on the Theory of Alternating Currents, in Vol. XIII. On page 502, line 13, omit the words "one-half of," and in line 16, read "also" for "however."

J. Hopkinson.

In Captain Sankey's paper on Electrotyping, in Vol. XIV., footnote at bottom of page 33, for "$\frac{\text{Current}}{372}$" read "$\frac{\text{Current}}{384}$."

JOURNAL

OF THE

SOCIETY OF

Telegraph-Engineers and Electricians.

Founded 1871. *Incorporated* 1883.

Vol. XIV. 1885. No. 57.

The One Hundred and Forty-third Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, 26th March, 1885—C. E. Spagnoletti, Esq., M. Inst. C.E., &c., President, in the Chair.

The Secretary read the minutes of the last meeting.

The following transfer from Student to Associate was announced:—Frederick William Ford.

Donations to the Library were announced as having been received since the last meeting from the following:—The Board of Trade; Mons. E. Mascart; J. W. Pearce, Esq.; T. Ishie, Foreign Member; Professor Silvanus Thompson, Member.

A hearty vote of thanks was accorded to the donors.

The President announced that the Executive Council of the International Inventions Exhibition had been good enough to grant to members of the Society the privilege of having season tickets to the Exhibition at half-price, and intimated that a circular would be issued by the Secretary, giving details.

The following paper was then read:—

ON THE SEAT OF THE ELECTRO-MOTIVE FORCES IN A VOLTAIC CELL.

By Professor OLIVER J. LODGE, D.Sc.

CONTENTS.—1. Summary of the theoretical views which can be agreed to, and of the remaining points in dispute.—2. Views of Clerk Maxwell.—3. Argument that the Peltier effect does not necessarily measure electro-motive force at junctions.—4. Hydraulic illustrations of the difference between Peltier and Volta forces, according to the views of the writer.—5. Summary of condensed statements embodying orthodox views.—6. Statement of the writer's own views.—7. Example of the calculation of a Volta effect. 8. Attempt to mentally represent the action of atmosphere on metals at the instant of contact.—9. Calculation of a series of metal-air contact-forces, *i.e.*, of an absolute Volta series in air or water.—10. Calculation of Volta series in other media, such as chlorine and sulphur.—11. Discussion and hypothetical explanation of true (or thermo-electric) contact-force, and reason why for insulators it is large, for metals small.—12. Thermo-electric contact-forces between metals and electrolytes, and theory of the common or simple voltaic cell.—13. Summary of condensed statements embodying the writer's own views, first for substances in general, and then for metals.—14. Explanation of the last of the above statements. Example of a Peltier series, and of a Thomson series; and question whether thermo-electric contact-force depends upon chemical tendency or whether it is purely physical.—15. Brief summary of the argument relating to the seat of electro-motive force in the pile.

1. A long summary of the work that has been done in the observation and determination of what are called "contact-forces," from the time of Volta to the present, is printed in the *Philosophical Magazine* for the current month. I need not therefore repeat any of this, but, assuming it known, can proceed to the results of the survey, which may be summed up thus: (1) that there is certainly an E.M.F. at the junction of two different substances, or even of the same substance in two different states; and (2) that the total E.M.F. of a circuit is the algebraic sum of all such contact-forces at every junction in the circuit. I do not know that these two propositions could be passed *nem. con.*, but I believe that, provided they are properly understood, the dissenting minority would be a very small one.

We can also make a negative proposition which will command almost universal assent—viz., that if in the above two propositions, *instead of the* sum of the contact-force at every junction, we attend

only to the contact-forces at the *metallic* junctions, the propositions will no longer be true. This fact, that the metallic junctions are insufficient to account for all the E.M.F., was established completely by Becquerel, De la Rive, and others, and still more thoroughly and exhaustively by Faraday. It is the easiest possible thing to make a number of batteries which shall give a current without any metallic junction whatever. Faraday gives some thirty of them.*

One more certain proposition we can lay down, viz., that whenever a current is produced, the energy of the current must be maintained by absorption of heat, or by chemical action, or by gravity, or by some other such agent, not by mere contact.

So much being agreed to, what remains as subject-matter for controversy? This: a voltaic circuit contains at least three junctions; what is the value of the contact force at each of them, and especially to which junction is the major part of the observed E.M.F. due? Is it the zinc acid? or is it the copper acid? or is it the zinc-copper? There is no other question. The old chemical and contact controversy has died out, but another controversy remains. Most physicists probably would say to-day that the major part of the E.M.F. of the cell resides at the zinc-copper junction. This was Volta's view, and this is the view of the text-book writers taught by Sir William Thomson. Some few would say at the zinc-acid junction, and among them I must confess myself.

It is no question between contact and something else. It is a question between a passive inert metal-metal contact and an active energetic metal-fluid contact with potentialities of chemical action straining across the junction. What is there to distinguish between the two? Electrostatic experiments with air condensers

* *Exp. Res.*, ii., 2020. Dr. J. A. Fleming describes another of these batteries in *Phil. Mag.*, June, 1874, and gives some very cogent and readable arguments in favour of the "chemical theory" of battery E.M.F., suggesting that the difference of potential between the terminals of a battery on open circuit is due to potential chemical combination of the metals and electrolytes. He does not, however, explain the old Volta experiment; and, as Professor Chrystal has pointed out (*Ency. Brit.*, art. "Electricity," p. 99), upholders of the chemical theory are bound *to explain this.*

prove nothing. They add up three E.M.F.'s, air/M + M/M′ + M′/air, and give you the sum. The experimenters usually assume that M/M′ is what they are measuring, but there is no proof to be given in support of the assumption, except that if you substitute water for air the effect remains almost unaltered; but then water contains oxygen as the active element, the same as air does. Well, then, it may be urged, the effect is the same in vacuo and in hydrogen as in air; and to this I answer, Not proven.

Can any further assertions be made with reference to electroscopic experiments as bearing on voltaic theory? Yes; it can be asserted that by adding up the Volta effects for A/B, for B/C, for C/D . . ., and for Z/A, you arrive at the total E.M.F. of the circuit A, B, C . . . A. True; but what then?

The Volta effect you call A/B is really	air/A + A/B + B/air;
that you call B/C is	air/B + B/C + C/air;
. . . . .	. . . . . .
and that you call Z/A is	air/Z + Z/A + A/air.
Add them up and you get	A/B + B/C +. . . + Z/A,

which *must* be the E.M.F. of a current by common sense—*i.e.*, without violent experimental disproof, which no one has ever attempted to give. This fact, that the sum of the Volta effects equal the sum of the true forces in a closed circuit of any materials, has nevertheless caused persons to suppose that air/metal forces are negligibly small. But it is clear that they may have any value they like without affecting the truth of the law. The only way they could affect it would be for air/M not to be equal to − M/air. The experimental proof of the summation law, therefore, establishes that air/M *is* equal to − M/air, as well as the important fact that the contact-force at each junction is independent of all other junctions of what kind soever.

2. Leaving electrostatic determinations as without bearing on the point at issue, let us ask, Is there no direct and straightforward way of measuring the actual E.M.F. at a particular junction without disturbance from other junctions? The answer is most clearly given by Clerk Maxwell, thus:—*

* "Electricity and Magnetism," Vol. I., art. 249. Abbreviated above *because so easy* of reference.

"Sir W. Thomson has shown that if Π is the coefficient of Peltier effect, or the heat absorbed at the junction by unit current in unit time, then J Π is the E.M.F. at that junction acting with the current. This is of great importance, as it is the only method of measuring a local E.M.F.; the ordinary method of connecting up by wires to an electrometer being useless. This Peltier measurement is quite independent of the effect of contact-forces in other parts of the circuit. But the E.M.F. does not account for Volta's force, which is far greater and often opposite. Hence, the assumption that the potential of a metal is to be measured by that of the air in contact with it must be erroneous, and the greater part of Volta's E.M.F. must be sought for, not at the junction of the two metals, but at one or both of the surfaces which separate the metals from the air or other medium which forms the third element in the circuit."

And in another place he says:—*

"In a voltaic circuit the sum of the E.M.F.'s from zinc to electrolyte, from electrolyte to copper, and from copper to zinc, is not zero, but is what is called the E.M.F. of the circuit—a measurable quantity. Of these three E.M.F.'s only one can be measured by a legitimate process—that, namely, from copper to zinc. If we cause an electric current to pass from copper to zinc, the heat generated in the conductor per unit of electricity is a measure of the work done by the current, for no chemical or other change is effected. Part of this heat arises from the work done in overcoming ordinary resistance within the copper and the zinc. This part may be diminished indefinitely by letting the electricity pass very slowly. The remainder of the heat arises from the work done in overcoming the E.M.F. from the Zn to the Cu, and the amount of this heat per unit of electricity is a measure of the E.M.F. Now, it is found by thermo-electric experiments that this E.M.F. is exceedingly small at ordinary temperature, being less than a microvolt, and that it is from zinc to copper.† Hence the statement deduced from experiments in which air is the third

* Maxwell. Letter to the *Electrician*, April 26, 1879.

† Further on (§ 14) I point out that this statement is not quite true, but it does not affect the main argument.

medium, that the E.M.F. from copper to zinc is ·75 volt, cannot be correct. In fact, what is really measured is the difference between the potential in air near the surface of copper and the potential in air near the surface of zinc, the zinc and copper being in contact. The number ·75 is therefore the E.M.F., in volts, of the circuit copper, zinc, air, copper, and is the sum of three E.M.F.'s, only one of which has yet been measured."

With every word of Maxwell I cordially agree.

3. Pellat, however, considers the Peltier effect to be quite distinct from and have no relation to the true E.M.F. of contact. In explaining this he makes use of a piece of unpleasantly plausible reasoning, which I myself have heard Professor Ayrton use, and which, when unexpectedly suggested, is so painfully benumbing that it is worth while to quote it and to indicate its weak point. Pellat's statement of the argument is rather long; perhaps it can with advantage be abbreviated.

Two metals, A and B, put into contact are at different potentials, the difference A/B being due to and equal to the E.M.F. of contact. There is then at the junction, not only the contact-force E, but also the equal opposite force $-dV$, due to the difference of potential established. Either of these forces alone would resist or aid the passage of electricity across the junction, and so give rise to a Peltier effect, but both together will do nothing of the sort; and so, if there be any Peltier effect, it must be some small residual phenomenon, or it must be due to some other and totally distinct cause.*

Professor Ayrton's way of putting the argument, which I think he said he got from Sir William Thomson, is something like this. When Q units of electricity are transmitted against a force E, work E Q is done; also when they are transmitted up a difference of potential $V' - V$, work $Q(V' - V)$ is done; but, in an open circuit containing an electro-motive junction, $V - V'$ is produced by and is equal to E. Hence, at an electro-motive junction no

* Thus it may be, suggests Pellat, due to a slight difference between E and $-dV$, produced by the mere fact of a current passing—*i.e.*, contact E.M.F. with electricity at rest may be slightly different to what it is with electricity *in motion.*

work need be done by a current: in other words, the existence or non-existence of a Peltier effect has nothing to do with the existence or non-existence of a local E.M.F.

The fallacy of the argument in this latter form lies in the over-precise spécification of locality; it gratuitously asserts as true for the *junction* what is only proved to be true for the whole circuit. And the former mode of stating the argument likewise erred in assuming that there could be no work done *at a junction* if it were perfectly easy to drive electricity either way across it—*i.e.*, if there were no work done *on the whole.*

4. To exhibit the fallacy, consider a hydrostatic analogy. Two vessels of water connected by a pipe in which is a motor of some kind, which without leakage exerts a specified force on the water and maintains a constant difference of potentials, but then remains stationary, doing no further work. We typify it feebly in the diagram by an impracticable close-fitting water-wheel driven by a weight without friction.

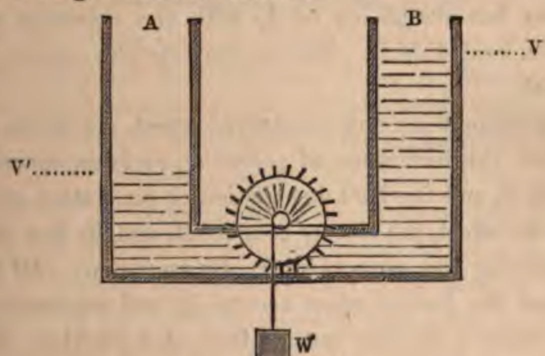


FIG. 1.

Hydrostatic analogue of the true contact or Seebeck force, and of the real though small difference of potential which it maintains between two metals in contact. W is a weight driving a water-tight wheel until stopped by the difference of potential set up. The hydraulic raising or lowering of the weight represents the Peltier effect.

$V - V'$ is the equivalent of the force exerted at the junction, and everything is in equilibrium. It is perfectly easy for water to flow from one vessel into the other under the influence of the slightest extra force, for W helps the water up the hill $V - V'$, when the flow is in that direction; and whenever the flow is

reversed it lets the water gently down again, taking all its energy out of it. If water is made to flow from A to B, say, by pouring more into A, the weight W is lowered or energy disappears (heat absorbed) at the junction; if it is made to flow from B to A the weight is raised, or energy (say heat) is generated at the junction. Thus there is a true Peltier effect at the junction, despite the existence of $V - V'$ and its equality to the junction force, and yet no resistance is offered to the flow of water either way. Thus is the first form of the argument controverted.

To pump water from A to B by any other pipe would need work to be done equal to $Q(V - V')$, and to pump water against the force of W, acting alone, would also need work $E Q$; but when the water goes from A to B *viâ* W, or *vice versâ*, no work is done on the whole. Quite true: but the conclusion that no work is done *at the junction* by no means follows. Work *must* be done at the junction in proportion to the force there (by inspection of the diagram), and accordingly the existence or non-existence of a Peltier effect has *everything* to do with the existence or non-existence of a local E.M.F. This controverts the second form of the argument.

If the argument be now considered upset, are we to proceed to assert that the difference of potential, or force, concerned in the Volta effect, and the heat destruction or generation concerned in the Peltier effect, are closely connected, and in fact different ways of observing the same thing? By no means. All we have proved is that the Peltier effect accurately and necessarily represents and measures the true contact force at a junction. True, we have considered a difference of potential $V - V'$ as produced by this contact-force in an incomplete circuit, and so it is; but nothing has been said to imply that this difference of potential has anything to do with what is observed in electrostatic experiments as the Volta effect. So far from this I will assert that what is usually observed when two metals are touched and separated is not primarily a difference of potential between the metals at all. They are at different potentials when separated, no doubt, because they are oppositely charged; but they may have been at the same potential until separated. The real Volta effect is almost

independent of the true contact-force, and of the difference of potential which it produces. In other words, a good Volta effect can be observed when there was no difference of potential whatever between the metals when in contact.

According to my view the Volta effect is produced, not by a contact-force at the junction of the two metals, but by a contact-force at their free surfaces, between the metals and the air or other medium surrounding them. To represent this hypothesis

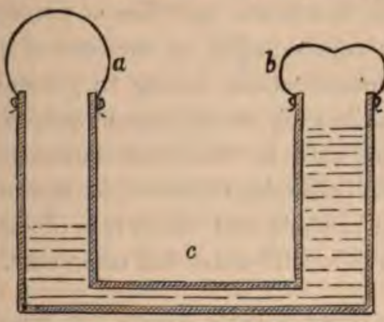


FIG. 2.

Hydrostatic analogue of the Volta effect, or *apparent* difference of potential produced by metallic contact, and of the opposite charges but uniform potential which it maintains between the metals in contact. The vessels are covered by air-tight elastic bags differently stretched.

by a hydrostatic model we shall have to maintain the difference of level in the two connected vessels, not by a force at the junction, but by a force at the surfaces; say, by using closed vessels and compressed air, or, more pictorially, by differently stretched elastic membranes or bladders tied over the tops of the vessels.

Note that the difference of level in this case implies no difference of potential, and, as before, no work is required to transfer water between *a* and *b*. Hence it is not easy to distinguish this case from the former, and this difficulty of distinguishing between the two cases is what has given rise to most of the confusion. The only easy criterion is the non-existence in the second case of any Peltier effect at the junction *c*. Naturally it is possible and common for the two effects to be superposed, but they are essentially independent.

Since the two vessels in the second case are at the same

potential, the way to observe the effect is to cut and seal the pipe at *c*, and then show that the vessels are differently charged, which is what Volta did. The model does not indeed represent the gradual change of potential induced as the distance between the condenser plates increases, and it is scarcely worth while to complicate the matter by making a more elaborate model. The thickening of the dielectric layer of a condenser, when its plates are separated, corresponds exactly to the thickening and strengthening of an elastic membrane, and rise of potential in the one case is accurately representable by increase of pressure in the other; but such considerations belong to general electrostatics, and have no special bearing on our present subject.

5. The following notes or condensed statements are intended to be critically exact (allowing, of course, for mistakes and possible slips), so as to bear analysis, and hence it is probably worth while to reproduce them here with notes and comments.

I.—Orthodox Statements believed by the Writer to be True in the form here set down.

A.—Volta.

i. Two metals in contact ordinarily acquire opposite charges. For instance, clean zinc receives a positive charge by contact with copper, of such a magnitude as would be otherwise produced under the same circumstances by an E.M.F. of about ·8 volt.*

ii. This apparent contact E.M.F., or "Volta force," is independent of all other *metallic* contacts wheresoever arranged; hence the metals can be arranged in a numerical series such that the "contact-force" of any two is equal to the difference of the numbers attached to them, whether the contact be direct or through intermediate metals. But whether this series changes when the atmosphere, or medium surrounding the metal, changes, is an open question. On the one side are experiments of De la Rive, Brown, Schultze-Berge; on the other side, of Pfaff, Pellat, Thomson, Von Zahn. It certainly changes when the free metallic

* Observe that it is not said that two metals in contact acquire different potentials. Such difference of potential I believe to be only apparent. Compare *Figs. 1 and 2.*

surfaces are in the slightest degree oxidised or otherwise dirty. And in general this "Volta force" is very dependent on all non-metallic contacts.

iii. In a closed chain of any substances whatever, the resultant E.M.F. is the algebraic sum of the Volta forces measured electrostatically in air for every junction in the chain—neglecting magnetic or impressed E.M.F. [Verified most completely by Ayrton and Perry.]

B.—Thomson.

iv. The E.M.F. in any closed circuit is equal to the energy conferred on unit electricity as it flows round it.

[Neglect magnetic or impressed E.M.F. in what follows.]

v. At the junction of two metals, any energy conferred on, or withdrawn from, the current must be in the form of heat. At the junction of any substance with an electrolyte, energy may be conveyed to or from the current at the expense of chemical action as well as of heat.

vi. In a circuit of uniform temperature, if metallic, the sum of the E.M.F.'s is zero by the second law of thermo-dynamics; if partly electrolytic, the sum of the E.M.F.'s is equal to the sum of the energies of chemical action going on per unit current per second.

vii. In any closed conducting circuit, the total intrinsic E.M.F. is equal to the dynamical value of the sum of the chemical actions going on per unit electricity conveyed ($\Sigma \cdot J\theta\epsilon$), diminished by the energy expended in algebraically generating reversible heat.

viii. The locality of any E.M.F. may be detected, and its amount measured, by observing the reversible heat or other form of energy there produced or absorbed per unit current per second. [This is held by Maxwell, but possibly not by Thomson,* though its establishment is due to him.]

* The only reason which I can think of as likely to have caused Sir William to doubt or deny the validity of this proposition is given, and I hope refuted, at *sections 3 and 4.*

II.—Statements believed by the Writer to be False, though Orthodox.

ix. Two metals in air or water or dilute acid, but not in contact, are practically at the same potential. [Sir Wm. Thomson, Clifton, Pellat.]

x. Two metals in contact are at seriously different potentials (*i.e.*, differences of potential greater than such millivolts as are concerned in thermo-electricity). [This is held by nearly everybody.]*

xi. The contact force between a metal and a dielectric, or between a metal and an electrolyte such as water and dilute acid, is small. [Ayrton and Perry, Clifton, Pellat, and probably Sir Wm. Thomson.]

6. Before proceeding to the statements embodying my own views, it may be more interesting if I try to explain in a fuller and more connected manner what they are.

Let us regard the air as a dielectric bath of oxygen, in which metals are immersed, and picture a piece of zinc surrounded by oxygen molecules which are straining at it, and endeavouring to combine with it. They may indeed partially succeed; but suppose they do not, we have here a strong potential chemical action or chemical strain, which must probably be accompanied by some physical phenomenon. Now remember that oxygen is an electro-negative element; and without endeavouring to examine too precisely what signification is involved in that statement, it will be not out of accord with orthodox views if we assume that it means that at least any dissociated oxygen atoms are negatively charged, each with the characteristic charge of a free dyad atom. Granting something equivalent to this, without

* It is much more natural to suppose that the potential of a metallic conductor is uniform, whether it is homogeneous or not. Indeed, it is not only more natural, but it is true, that two parts of a conductor *can* only differ in potential by reason of an E.M.F. located at the junction. Now there usually is an E.M.F. at a junction, but it is only of such a magnitude as is concerned in thermo-electricity. This, indeed, does produce a difference of potential between the metals, but nothing else can.

Both Mr. Sprague and Mr. Heaviside have published statements very much in accord with what I consider the true view of the activity of the pile.

pressing the form of expression too closely, we perceive that the strain of the oxygen towards the zinc will result in what I metaphorically call a slackening up, or attempted compression, of the negative electricity in it, *i.e.*, to a rise of negative potential. We may therefore say that zinc is at a lower potential than the air surrounding it, and that the step of potential in crossing the boundary from zinc to air is closely connected with the chemical affinity between zinc and oxygen. Observe that this step of potential does not obviously nor probably depend on the amount of oxygen present. It is possible that a few million molecules may be as effective as a large number. Note also that the step of potential is by no means caused by actual oxidation: in so far as the zinc surface is tarnished by oxidation the strain will be diminished and the step of potential become less.

Nothing is said here about the possible effect of the nitrogen, because it is simplest in the first instance to ignore it, though whether experiment will justify this simplicity or not, I do not yet know.

We may go further, and assert that if in general the chemical affinity of two substances can be measured by their energies of combination, then the step of potential in the present case may perhaps be calculable from the heat of combustion of zinc.

And one may justify this assertion thus. Let an atom of oxygen combine with an atom of zinc; it will generate an amount of heat, h, and its characteristic charge, q, will be given up to the zinc and will thereby fall down the step of potential, v, which separates the zinc from the air. Now, if we suppose that the heat, h, is the representative and equivalent of the fall of energy, $q\,v$, it follows of course that

$$v = \frac{\mathrm{J}\,h}{q}.$$

Make the hypothesis and see what comes of it.

7. The oxidation energy of zinc per gramme-equivalent (*i.e.*, 65 grammes of zinc or 16 of oxygen) is, according to the determinations of Julius Thomsen, Andrews, and Favre and Silbermann, 85,430, 84,825, and 83,915 respectively.

The *amount of electricity* needed to deposit a gramme-

equivalent of zinc, or of any dyad element, is, according to the modern determination of Lord Rayleigh,* 19,320 units.

Hence the value of $\frac{h}{q}$, which is a ratio evidently independent of the number of atoms dealt with, lies between $\frac{85400}{19320}$ and $\frac{84000}{19320}$ probably. Let us say it is $\frac{85000}{19320}$, or 4·4.

Now J in absolute measure is 42×10^6; so the value of v, according to the above hypothesis, comes out $1{\cdot}85 \times 10^8$, that is, 1·85 volts.

This, then, I say, is the step of potential between zinc and air. To avoid circumlocution I will speak as if the above hypothesis were admittedly true, and all I now say stands or falls with it.)

All clean bright zinc is thus about 1·8 volts below the potential of the air near it: tarnished or oxidised zinc will exhibit less difference, and it is perhaps possible that perfectly oxidised zinc need show no difference of potential at all between itself and the air. The step of potential by no means therefore depends upon the occurrence of oxidation; it is the oxidation *tendency* which causes it, but so far as oxidation actually takes place the step diminishes.

Proceed to consider a piece of copper similarly. Oxygen molecules are straining at it also, but with less force. The combustion energy of copper per gramme equivalent is given by the three authorities already quoted as 37,160, 38,290, and 43,770 respectively. These do not agree well, and it is difficult to know which to take; but Thomsen's results are, I believe, generally relied on, so, assuming his, the step of potential between copper and air will be $\frac{{\cdot}42 \times 37200}{19320}$ volts—that is, about ·8 volt. This, then, is the amount by which clean bright copper differs from the air. Oxidised copper will differ less. Comparing this value for copper with that just obtained from zinc, we perceive that a piece of zinc and a piece of copper are, when separate, not at the same potential; they differ by about a volt from each other.

Now put the zinc and copper into direct metallic contact, and neglect for the present the third of a millivolt of E.M.F. developed

---

* "4·025 grammes of silver are deposited by an Ampère current in an hour." —*Montreal Address.* This gives the electro-chemical equivalent of silver ·01118, *and of hydrogen ·00010352.*

at the junction, which acts so as to drive positive electricity from copper to zinc. A rush of electricity must take place from the copper to the zinc to equalise their potential; it is impossible that they can remain at different potentials when directly united: all parts of a conductor must be at a uniform potential, and the rush has taken place because they were not so when put into contact.

8. Picturing to ourselves the effect as produced by the straining oxygen atoms, we shall perceive that they could not get at either metal when separate: first, because they surrounded it everywhere, and strained equally on all sides; and second, because being all charged with negative electricity they could not move in on all sides at once without, so to speak, compressing the electricity in the body and giving it an absolute charge. But directly the copper touched the zinc the oxygen atoms were cleared away at the point of contact, and the stress of those at the rest of its surface was no longer counterbalanced. Moreover, they *can* now all move nearer to the zinc, because a way of escape for electricity is provided into the copper, whose surrounding oxygen atoms will be thus driven back somewhat further from the surface; until the dielectric strain, assisting the chemical strain on the copper surface and opposing it on the zinc surface, prevents further displacement, and equilibrium is again attained. The electricity which escaped from the zinc to the copper was negative electricity (oxygen being essentially an electro-negative element); the negatively charged oxygen atoms have moved a little nearer to the zinc than their normal distance—*i.e.*, the thickness of its layer of negative electricity is reduced, or its surface is positively charged; the negative layer on the copper has been slightly thickened—its surface is negatively charged.

This is a pictorial way of representing the process, and may be regarded as somewhat fanciful; it is, however, the way in which the theory originally occurred to me, and it permits more insight into the processes than a mere statement in terms of potential can; though it may well be that the imagined processes are but distant likenesses of the real ones.

The *oxygen atoms have* moved nearer to the zinc: it is now

more easily oxidised than before; the copper, on the other hand, is by contact with zinc somewhat protected.

Observe that the contact has not developed any force; it has only, by sweeping away the oxygen from the point of contact, enabled previously existing forces to do work and produce their effect.

The air surrounding the metals in contact is in a state of slight dielectric strain, such as would be produced by two pieces of any one metal of similar size and position, charged so as to differ in potential from each other by a volt.

Zinc and copper plates in contact may therefore be regarded as the plates of a condenser, but they form a peculiar condenser, for they are not really at different potentials; the whole step of potential which throws the air into its state of dielectric strain is located on their bounding surfaces.

9. Let us now calculate a series of metal-air contact forces from the heats of combustion; remembering that all we have to do, in order to convert heats of combustion per dyad gramme-equivalent into volts, is to divide by $\frac{19320 \times 10^8}{42 \times 10^6}$, that is, by 46,000.

But the decision as to what numbers we shall take to represent heats of combustion is a matter of some difficulty, for not only do the numbers obtained by different observers for the same reaction differ, sometimes considerably, but it is not obvious, when different oxides are formed, which of them we are to consider as most applicable to the case of the Volta experiment. Perhaps one should take the most common or stable oxide; perhaps, seeing that no combination is supposed actually to occur, and since the metal is, so to speak, in excess, it is most reasonable to take the lowest oxide which the substance will form. It may be that the data are not known for this; it may even be that they have only been obtained for the hydrate instead of for the oxide.

I must therefore do the best I can, and quote several numbers wherever there is obvious doubt. I imagine that J. Thomsen's are the most reliable when they are available.

But it must be remembered all through, that since it is only the *tendency* to chemical action which is the cause of the Volta effect, whereas combination heats are obtained by permitting or

causing the combination to actually occur, the numbers obtained by calculation are not likely to be quite right; and they may be expected to err on the side of excess, the calculated number being higher than the actual value if directly observed.

Energy of Combination of Metals with Oxygen.

Metal.	Molecule.	Authority.	Heat of Formation.	The same reduced to volts.
Zinc	Zn,O	Thomsen	85,430	1·85
		Joule	77,000	1·66
		Andrews	84,825	1·84
		Favre and Silbermann	83,915	1·82
Tin	Sn,O	Andrews	67,100	1·46
Lead	Pb,O	Thomsen	50,300	1·15
	"	F. and S.	55,350	1·20
Iron	Fe,O	"	75,656	1·64
	Fe,O,H_2O	Thomsen	68,280	1·48
	$\frac{1}{3}(Fe_2,O_3)$	"	63,730	—
	$\frac{1}{4}(Fe_3,O_4)$	Andrews	66,448	1·44
Nickel...	Ni,O,H_2O	Thomsen	60,840	1·32
Cobalt...	Co,O,H_2O	"	63,400	1·38
Copper	Cu,O	"	37,160	·81
	"	Andrews	38,290	·83
	"	F. and S.	43,770	·95
	Cu_2,O	Thomsen	40,810	·89
	Cu,O	Joule	37,486	·81
Mercury	Hg,O	Thomsen	30,660	·67
	Hg_2,O	"	42,200	·92
Silver	Ag_2,O	"	5,900	·13
	"	F. and S.	12,226	·27
Hydrogen	H_2,O gas	Thomsen	68,360	1·49
	H_2,O liquid	"	78,000	1·70
	"	Dulong, Hess, Grassi, Joule, Berthelot	79,600	1·72
Potassium	K_2,O,Aq	Thomsen	164,560	3·56
	K_2,O	Woods	136,000	2·95
Sodium	Na_2,O,Aq	Thomsen	155,260	3·36
	Na_2,O	Woods	151,600*	3·28
	"	Hypothetical	130,000	2·82
Calcium	Ca,O	Thomsen	131,360	2·85
	Ca,O,Aq	"	149,400	3·24
Barium	Ba,O	"	130,380	2·84
	Ba,O,Aq	"	158,260	—
Strontium	Sr,O	"	130,980	2·84
	Sr,O,Aq	"	157,780	3·41
Magnesium ...	Mg,O	"	145,860	3·16
Palladium	Pd,O,H_2O	"	22,710	·50
Cadmium	Cd,O,H_2O	"	65,680	1·43
Thallium	Tl_2,O	"	42,240	·92
Manganese ...	Mn,O,H_2O	"	94,770	2·05
Aluminium ...	$\frac{1}{3}(Al_2,O_3,3H_2O)$	"	129,600	2·81
Lithium	Li_2,O,Aq	"	166,520	3·62
Arsenic	$\frac{1}{3}(As_2,O_3)$	"	51,530	1·12
Bismuth	$\frac{1}{3}(Bi_2,O_3)$	Woods	13,200	·29

* This number for sodic oxide agrees with Thomsen's value for the hydrated oxide, whereas for K, Ba, Sr, Ca, the oxide is distinctly below the hydrate. By analogy, one would expect to have to subtract some 25,000 from the hydrated *oxide* Na_2,O,Aq, *and* this gives the 130,000 which I put down as *a hypothetical number for* Na_2,O.

Numbers obtained from unsatisfactory oxides and hydrates, like those of aluminium, arsenic, and bismuth, are not likely to be useful for our present purpose. I know of no better data yet available, however.

It is now easy to write down a Volta series obtained by pure calculation from heats of combustion. We can then see how far it agrees with the results of direct experiment. The principle on which I determine which of the preceding numbers to select is simply to choose Thomsen's when it refers to the simplest oxide, and in other cases to take what one can get. Metals about which there is obvious uncertainty, as, for instance, sodium, aluminium, bismuth, etc., are omitted. I only take the common ones.

Calculated Volta Series.

Lithium and Magnesium ...	3·0	Nickel	1·32
Potassium	2·95	Lead	1·15
Calcium, etc.	2·84	Thallium	·92
Zinc	1·85	Copper	·81
Iron	1·64	Mercury	·67
Tin...	1·46	Palladium	·5
Cadmium	1·43	Silver	·13
Cobalt	1·38		

To compare this series with those obtained by experiment, we may as well take zinc as the metal of reference, and write down the Volta effect between it and the other metals, first as abstracted from the above table, and then as found by different observers. Strictly, one ought first to subtract Peltier forces from the observed numbers before comparing them with theory, but these forces are too small to make any appreciable difference.

Comparison of Calculation with Experiment.—Volta Effects in Air.

Metal Pair.	Calculated from Heats of Combustion.	Observed by				
		Pellat.		Ayrton and Perry.		Clifton.
		Clean.	Scratched	Using Commercial Zinc.	Using Amalgamated Zinc.	—
Zinc—						
Tin	·39	·25	·35	·28	·46	—
Lead	·70 or ·65	·15	·31	·20	·35	—
Iron	·21 or ·4	·56	·70	·60	·74	·75
Nickel	·53	·47	·63	—	—	—
Copper	1·04 or ·9	·71	·86	·75	·89	·85
Mercury ...	1·18	—	—	1·06	1·20	1·07
Silver	1·67 or 1·58	·91	1·12	—	—	—
Platinum ...	—	—	—	·98	1·13	—

The alternative calculated number sometimes given, is merely to show the kind of variation probable in those cases from uncertainty of data. In each case of agreement the calculated number is a little higher than the observed, as was to be expected. No reason occurs to me for the breakdown, and apparent interchange, in the case of lead and iron but such vague guesses as may occur to every one.

Measurements of the E.M.F. between clean metals plunged into distilled water or weak acid have been made by Clifton and Beetz.* I suppose one is justified in calling them—

Volta Effects in Water.

Metal Pair.	Calculated from Heat of Combination.	Observed by	
		Beetz.	Clifton.
Zinc—			
Copper	1·0	·98	·82 to ·92
Silver	1·7	1·23	—
Platinum	1·8 or less	1·52	1·3 (Smee)
Sodium Amalgam—			
Zinc	1·0	·78	—
Copper	2·0	1·79	—
Silver	2·6	2·05	—
Platinum...	2·8	2·31	—

It must be remembered that it is not relative numbers only that we have calculated, but absolute; and the fact that the heats of combustion reduced to volts are numbers of the same order of magnitude as the Volta effects, is of itself a strong confirmation of the belief that chemical strain at the air contacts is the real cause of the apparent contact-force at the junction of two metals.

The agreement of the numbers, though not exact, seems to me too close to be the result of accident. One may, I think, claim that the hypothesis whence the calculated numbers are obtained is justified by the figures as far as they go.

* *Beetz, Ann. der Physik*, X., 348, 1880.

Volta Effect in other Gases.

10. Not many measurements of metal pairs have been made, even in the air; for mere permutations such as copper-tin, tin-silver, etc., follow at once from the numbers given above, by Volta's series law; but in gases other than air one has at present no experimental guidance, beyond the barest qualitative one given by Mr. Brown, that copper-nickel reverses its sign when changed from air to hydrochloric acid, and that copper-iron is reversed in sulphuretted hydrogen. But satisfactory observation in these gases is difficult, because they not only *tend* to attack the plates, but they *do* attack them; and so a film is formed and everything is rendered uncertain.

Another complication results from the fact, that when metals are taken out of air and put into a foreign gas, they are already coated with a film of oxygen, and it is not clear in what way this will affect the action of the new gas. It may have to be replaced almost by substitution; the affinity to be considered in chlorine, for instance, being something like $M,Cl_2 - M,O$. In a compound like H,Cl, the hydrogen may have to be provided for, the resulting chemical strain being, for instance, $M,Cl_2 - M,O + H_2,O - 2\,(H,Cl)$; but the consideration of the hydrogen affinities will not affect *differences*, and therefore will leave comparisons with experiment unaffected. Taking the metals as clean, however, and without air films, we must suppose the following series to be right:—

Energies of Combination of Metals with Chlorine; and Calculated Volta Series in that Gas.

Metal.	Molecule.	Authority.	Heat of Combination.	Calculated Volta Series in volts.
Zinc	Zn,Cl_2	Thomsen	97,210	2·1
Lead	Pb,Cl_2	Thomsen	82,770	1·8
Iron	Fe,Cl_2	Thomsen	82,050	1·78
Nickel	—	No data	—	—
Copper	Cu,Cl_2	Thomsen	51,630	1·12
Mercury	Hg,Cl_2	Thomsen	63,160	1·38
Silver	2(Ag,Cl)	Thomsen	58,760	1·28
Hydrogen...	2(H,Cl)	Thomsen	44,000	·96
Potassium	2(K,Cl)	Thomsen	211,220	4·62
Sodium	2(Na,Cl)	Thomsen	195,380	4·24

This series will hold, as far as *differences* are concerned, for hydrochloric acid also; because, whatever effect the hydrogen affinity may have in changing the numbers, it will have the same effect on all.

It is easy to write down the hypothetical series in bromine and iodine in the same way:—

Calculated Volta Series in Bromine and Iodine.

Metal.	In Bromine.	In Iodine.
Potassium	3·85	3·50
Zinc	1·74	—
Lead	1·40	·87
Silver	1·10	·60
Copper	·89	·23
Hydrogen	·40	—·13

All this supposes the metals to be perfectly clean, and not covered with a film of a foreign gas like oxygen. On the hypothesis that the metal has been taken recently out of air, and that the film of oxygen with which it is covered has to be torn from the metal, though it was not actually combined with it, the Volta series in chlorine or hydrochloric acid would be quite different, and more like this:—

Hypothetical Volta Series of Air-coated Metals in Chlorine or HCl.

Silver	1·8	Copper	·9
Lead	1·2	Zinc	·85
Iron	1·1		

Unfortunately, I can get no data for the heat of chlorination of nickel, but assuming it not very different from iron, the above series gives copper nickel in the right order, as observed by Mr. Brown, whereas the other one did not. I have no experiments with which to compare the numbers.

Calculated Volta Series of Clean Metals in Sulphur or Sulphuretted Hydrogen.

Metal.	Heat of Sulpurisation per gramme-equivalent.	Volts.
Potassium	91,276	1·95
Zinc	41,880	·87
Iron	35,506	·76
Lead	19,112	·41
Copper	18,266	·39
Silver	11,048	·34
Hydrogen	5,482	·12

But it will be observed that this is nothing like the order observed in sulphuretted hydrogen; it is popularly known that copper is more easily sulphurised in this gas than iron. Now, assuming that the metal had been covered with air films, and that the oxygen of this has to be replaced by sulphur, the chemical tendency, instead of being M_2S, is something more like $M_2S - M_2O$, or possibly $M_2S - M_2O + H_2O - H_2S$; and either of these will give a quite different order. Data are given on p. 624 of Naumann's "Gmelin-Kraut," Vol. I., for the neutralisation heats of various bases by H_2S, such as $Cu_2O + H_2S$, etc. These are something like what we want, and from them we reckon the following:—

Hypothetical Volta Series, in Sulphur or Sulphuretted Hydrogen, of Metallic Oxides, and possibly of Air-coated Metals.

Metal.	Heat of Reaction $M_2O + H_2S = M_2S + H_2O$.	Volts.
Silver	55,800	1·20
Mercury	48,700	1·04
Copper	31,600	·68
Lead	26,600	·57
Zinc	19,200	·41
Iron	14,600	·31
Sodium	7,700	·16

The series so obtained gives copper and iron in their proper order; but it is scarcely likely to be really correct, because it assumes that the *oxides* of the metals are exposed to the gas *rather than the* metals themselves. It is quite possible that it is

not very incorrect for tarnished metals—*i.e.*, metals coated with a film of oxide; but for ordinary clean metals, coated, not with a film of oxide, but with a film of oxygen, it is nothing but a rough approximation, given because we have no better data.

It is to be noted that, as the film of oxygen diffuses away, the Volta effect depending on it must diminish, until at length the active affinity causing the chemical strain is nothing more than M,S, or perhaps M,S − H_2,S. A gradual falling-off and ultimate even reversal of sign was observed by Mr. Brown in both H,Cl and H_2,S. In so far as actual chemical action occurs and a film of chloride or sulphide forms, so far of course also will the effect diminish; because it depends essentially on the unsatisfied chemical strain, not on the accomplished chemical action.

For a summary of the views here expressed, see section 13.

11. Having now explained why I believe the main part of the Volta effect to take its rise at an E.M.F. of contact between metal and medium, rather than between metal and metal, it remains to consider whether this belief requires one to assert that there is no true contact-force at all at the junction of two metals. By no means—the existence of such a force is undoubted; but for metals it is usually very small, and may be neglected in comparison with the Volta force, though, strictly speaking, what is observed electroscopically is a mixture of the two. It is this true contact-force which gives rise to the Peltier effect, and its variation with temperature (assisted by the Thomson effect) causes thermo-electric currents. A contact-force exists, as Thomson has shown, not only at the junction of two different metals, but also between parts of the same metal at different temperatures.

In another place* I have endeavoured to gain some insight into the nature of this true contact-force, and to suggest its cause. This has been done by many others; but I may be permitted to repeat my own notion—vague and incomplete though it avowedly is.

Molecules of matter do not move in independence of electricity; at any rate the converse is certainly true—electricity does not

* *Phil. Mag.*, December (suppl.), 1876, "On a Mechanical Illustration of Thermo-electric Phenomena."

move independently of matter. Electricity, in flowing through a wire, meets with resistance; there is something analogous to friction between the matter and the electricity, and the opposing force is precisely proportional to the strength of the current. This much is expressed by Ohm's law, $E = RC$, which is a carefully verified through empirical statement. But, analysing R into specific resistance of material (ρ) and sectional area of conductor, and permitting ourselves to regard $\frac{C}{\text{area}}$ as proportional to the velocity of electricity in a circuit of different thicknesses, we perceive that Ohm's law means that

$$\frac{dV}{dx} = \rho \times \text{velocity}.$$

Let us then postulate, between electricity and any given kind of conducting matter, a connection which shows itself as an E.M.F. proportional to the speed of their relative motion and to the specific resistance of the material. Molecules of matter are not at rest, but (say) vibrating at a rate depending on, or rather itself determining the temperature. These motions cannot be independent of electricity, but they result in no force urging it to flow, because their motions are symmetrical. But place two metals in contact—one hot, the other cold; or one copper, the other iron—at the junction, symmetry disappears: there must be constraint and accommodation; and, in whatever precise way this acts, it seems probable that it can be conceived of as having the same effect as a layer of molecules moving faster on their outward journey than on their return. If any such dissymmetry of velocity were produced, it would exert a propelling force on electricity* in the direction of the greatest velocity, because the force is proportional to the velocity. This is the way in which I picture to myself the Seebeck or true contact-force—the cause of thermo-electricity and of the Peltier phenomenon.

But now, why is this force so small in ordinary metals? Because it depends on ρ, the specific resistance, and this is small.

* I do not say necessarily on *positive* electricity. It seems a complication, but Sir William's researches show that it is positive in some metals and negative in others. In the case of lead only does the grip on both electricities *eem the same.*

Choose badly-conducting metals like bismuth and antimony, or, still better, selenium and tellurium, and the force will be greatly increased. Choose so-called non-conductors, like glass and silk ebonite, and it becomes enormous. But when one uses non-conductors we cannot expect to excite currents flowing in closed circuits—we can only expect electrical displacement and electrostatic phenomena; and indeed it is no such easy matter for electricity to move in such substances, even though the force urging it be excessive; and a little mechanical violence (friction) may be necessary to help it to move. But remember that no amount of friction can determine the motion in one direction rather than another: working the pump barrel exhausts no air unless there are valves. Friction may supply some of the energy, but the directing force must be in the substances in contact.* To assist the passage it is customary in electrical machines to touch together a conductor and insulator rather than two insulators. I doubt not that when metal touches glass the surface of contact would become chilled as soon as any transfer of electricity were really produced by the force; but the heat developed, by the friction apparently necessary to aid the transfer, effectually masks any chilling.

The measurement of contact-forces in the case of insulators is beset with difficulties, because it is so difficult to make electricity pass across the junction. No limit to the force has at present been observed; whenever an electrical machine reaches its limit and refuses to charge its prime conductor or a Leyden jar to a higher potential, it is accounted for by saying that the rate of leakage is now equal to the rate of production (which is undeniably true), but nothing is said about whether the rate of production is the same as it was when the jar was uncharged. It is a difficult matter to settle, because most of the leakage takes

---

* Mr. Joseph Thomson (*Proc. Roy. Soc.*, 1876) endeavours to extend ordinary contact methods to non-conductors. He was hardly likely to get very clear results; but he was able to find some definite electrical transfer as the result of mere contact, if it be admitted that it is possible to apply mere contact and no sort or kind of violence, a supposition which is probably inadmissible; and the least violence *destroys all novelty* and sends us back to Thales.

place close to the rubber; and, though it is quite possible, it is unlikely, that a limit to the force will be discovered by the true rate of production of a frictional machine being found less at high potentials than at low. When the substances in contact are two metals, it is impossible for them to drive electricity very hard, for it would, so to speak, slip through their fingers; but when an insulator is concerned, its grip is so great that probably there is no limit to the force until its insulating power is overcome, and through it also electricity begins to slip. Certainly any upper limit must be a very high one, for the force can readily pile up a charge till it produces sparks a foot or more long.

Whether *Volta* forces, or contact-forces between substances and the medium surrounding them, exist for insulators also, we do not know: we have no reason whatever to deny their existence; but whereas in the case of metals these exceeded the forces acting between the substances themselves, here in the case of insulators they are absolutely negligible by comparison. For intermediate instances they may have correspondingly important values, and it seems not unlikely that at the junction of metals with electrolytes, and of electrolytes with one another, the total contact force may be a complex one—partly chemical, and due to the possibilities of chemical action straining across the junction; partly physical, and due to different velocity of the molecules.

12. The preliminary experiments of Bouty have caused him to believe in the existence of physical contact-forces, at the junction of metals with electrolytes, which cannot be brought into harmony with energies of chemical action. And though the subject is too unexplored in this direction to be ripe for discussion, it may be well to point out that these contact-forces are important in the theory of the Volta pile, even in its simplest form.

Why is the E.M.F. of a zinc-copper battery less than that of a zinc-platinum?

Why is the E.M.F. of a zinc-lead or -iron battery smaller than either?

The same chemical action goes on in each—zinc is dissolved *at one end* and hydrogen liberated at the other; how then can

the E.M.F. be different if it is calculable from the chemical reactions?*

If we picture to ourselves the actual forces in action, we shall get a kind of answer indicated to us. In a zinc-iron cell the E.M.F. is due to the zinc pulling at oxygen harder than the iron does; but since the iron does pull too, with no inconsiderable strength, the balance of force is not so great as if the iron were replaced by copper, which pulls less, or by platinum, which barely pulls at all until it is coated and alloyed with hydrogen.

This answer cannot be considered as complete, and, in order to complete it, consider a more precise experiment.

Arrange a series of common dilute acid voltameters with their plates respectively zinc-zinc, zinc-iron, zinc-copper, and zinc-platinum. Pass one current through the series from zinc to the other metal, and measure the differences of potential between the plates in each cell. Now the same chemical action is going on in each. In each, zinc is dissolved at one side and hydrogen evolved at the other—the only difference being that it is liberated from surfaces of zinc, iron, copper, platinum, in the four cases. Now what is to prevent the E.M.F. between the terminals of each voltameter from being the same? But it is not the same (*pace* Prof. Exner). The zinc-zinc cell shows the greatest difference of potential between its terminals, the zinc-iron less, and the zinc-platinum may easily show a reverse difference, because it helps the current on instead of hindering it. It will be understood that the *precise* behaviour of the cells is determined by the intensity of the current (*i.e.*, current per area)—if it is weak, even the zinc-iron cell may help it on, but the zinc-platinum will help it on most; if it is very strong, even the zinc-platinum will retard it, but the others will retard it more, and the zinc-zinc most.

* Professor Exner cuts this knot in characteristic fashion, by asserting roundly that the E.M.F. of all such cells is the same, and that it matters nothing what metal is opposed to the zinc of a cell so long as it does not alter the chemical action going on. He further asserts that all batteries are non-polarisable and quite constant, as soon as they have got rid of dissolved air and before sulphate of zinc has accumulated. He verifies these extraordinary statements, to three significant *figures*, by straightforward experiment.

Now, why is all this? Take the difference between the heats of formation of Zn,SO_4 and the decomposition of H_2,SO_4, at the comma, and you will have the total energy liberated by the current in each cell, in addition to the mere RC^2 frictional generation of heat common to all conductors. This energy is the same in all the cells, but not in all does it take the same form. In the zinc-platinum cell it mainly results in driving the current forward. In the zinc-zinc cell it wholly results in a Peltier (or Joule and Bouty) generation of heat. In going out of the cell by the cathode zinc plate, it has to move hydrogen towards it, and (*ipso facto*) oxygen away from it, in opposition to the strong chemical attraction; thus it will do work and generate energy, which, since there is nothing better to do, must exhibit itself as heat. At the iron surface less heat is generated, and at the copper less still; but at any cathode which attracts oxygen, some heat must be generated by a current made to do work in opposition to this attraction.

In the zinc-zinc cells there is no propulsion of electricity at all by the cell: on one side, where the current enters, zinc is dissolved and the current helped forward with the full energy (or nearly the full energy) of the combination, so that no (or nearly no) waste energy or heat is there produced; but on the other side, where the current leaves, the same combination is (not exactly undone but) opposed, and the current hindered with (probably something less than) the full energy of the combination, and there the heat of combination is generated.

Thus, regarding the passage of hydrogen to the cathode as a virtual separation of O (or SO_4) from it, we may say in general that in any one of the above cells, used as a voltameter, the energy available for helping the current on is that represented by the difference between the combination energies of the substances respectively attacked and liberated—*i.e.*, $Zn,SO_4 - H_2,SO_4$; but that besides this the combination M,SO_4 is virtually undone, and since its energy appears as a generation of heat at the cathode, it is so much to be subtracted from the propelling force available for the current, only the balance being left for this purpose, *viz.*, $Zn,SO_4 - H_2,SO_4 - M,SO_4$.

13. I can now continue the quotation of the remainder of the condensed notes with the certainty that they will be at any rate intelligible to those who have followed the preceding. I begin with statements intended to be true for substances of every kind, and then specialise them for the case of metals.

III.—Statements believed by the Writer to be True, though not Orthodox.

xii. A substance immersed in any medium tending to act upon it chemically will (unless it is actually attacked) be at a different potential to the medium in contact with it—positive if the active element in the medium is electro-positive, negative if the active element is electro-negative.

xiii. The above difference of potential can be calculated approximately from the potential energy of combination between the substance and the medium: the energy being measured by compelling the combination to occur, and observing the heat produced per amount of substance corresponding to one unit of electricity.

xiv. In addition to this contact-force, due to potential chemical action or chemical strain, there is another which is independent of chemical properties, but which seems to be greatest for badly conducting solids, and which is in every case superposed upon the former contact-force, the two being observed together and called the Volta effect. Very little is known about this latter force, except in the case of metals; and in these it varies with temperature, and is small. In the case of non-metals it is often much larger than the chemical contact-force.*

xv. The total contact-force at any junction can be experimentally determined by measuring the reversible energy developed or absorbed there, per unit quantity of electricity conveyed across

* I here assume, what I suppose is recognised as true, that what is known as frictional generation of electricity is really due to a contact-force between the substances rubbed—a force which is exceedingly great for insulators. See section 11. Davy seems to have held this view, from a note on p. 50 of his Bakerian lecture in 1806.

the junction. [Practical difficulties, caused by irreversible disturbances, being supposed overcome.]*

xvi. In a chain of any substances whatever, the resultant E.M.F. between any two points is equal to the sum of the true contact-forces acting across every section of the chain between the given points (neglecting magnetic or impressed forces).

xvii. In a closed chain the sum of the "Volta forces," measured electrostatically in any (the same) medium, is equal to the sum of the true contact-forces; whether each individual Volta force be equal to each individual true force or not. (See section 1.)

xviii. Wherever a current flows across a seat of E.M.F., there it must gain or lose energy at a rate numerically equal to the E.M.F. multiplied by the strength of the current.†

Development of the above, and Special Application to Metals.

xix. A metal is not at the potential of the air touching it, but is always slightly below that potential by an amount roughly proportional to its heat of combustion, and calculable, at any rate approximately, from it. For instance, clean zinc is probably about 1·6 volts below the air, copper about ·8 volts below, and so on. If an ordinary oxidising medium be substituted for "air" in the above statement, it makes but little difference.

xx. Two metals put into contact reduce each other instantly to practically the same potential, and consequently the most oxidisable one receives from the other a positive charge, the effect of which can be observed electrostatically.

xxi. There is a slight true contact-force at the junction of two metals which prevents their reduction to *exactly* the same poten-

* These difficulties are, however, tremendous for most substances except metals. M. Bouty's is the only attempt I know of to examine junction energy between metals and solutions of their salts, which is the case next in simplicity to metals. Observe that the statement says *energy*, not heat only.

† A current gains energy at any junction at which heat is absorbed, or chemical combination permitted, or any other form of energy transformed, by the passage of the current. The current gains the energy which has in the other form disappeared.

A current loses energy at a point where it causes other forms of energy to make their appearance—*e.g.*, generation of heat, decomposition of chemical *compounds, etc.*

tial, but the outstanding difference is small and varies with temperature. It can be measured thermo-electrically by the Peltier effect, but in no other known way. It is probably entirely independent of surrounding media, metallic or otherwise.*

xxii. If two metals are in contact, the potential of the medium surrounding them is no longer uniform : if a dielectric, it is in a state of strain; if an electrolyte, it conveys a current.

xxiii. In the former case the major part of the total difference of potential is related closely to the difference of the potential energies of combination, and is approximately calculable therefrom. In the latter case the total E.M.F. is calculable accurately from the energy of the chemical processes going on, minus or plus the energies concerned in reversible heat effects.†

xxiv. There are two distinct and independent kinds of series in which metals (and possibly all solids) can be placed: one kind depends on the dielectric or electrolytic medium in which the bodies are immersed, the other kind depends on temperature. The one is the real Volta series, but it is the commonly observed Volta series minus the Peltier; the other is the Peltier or thermo-electric series. To reckon up the total E.M.F. of a circuit, we may take differences of numbers from each series and add them together.

14. It is necessary to illustrate the meaning of this last statement, No. xxiv. By "real Volta series," I mean such series as we have attempted to calculate from purely chemical data, because they depend on chemical tendencies. By "Peltier or thermo-electric series," I mean those giving a purely physical E.M.F., produced we know not quite how, whose energy-source is not

---

* To distinguish between Peltier force and Volta force henceforward, it will be best to write Bi/Sb or Zn/Cu for the former, and Zn/Air/Cu or Fe/Water/Pt for the latter. The force electroscopically observed is Air/Zn/Cu/Air, but this involves both; the right way of denoting the Volta effect pure and simple is Zn/Air/Cu.

† Such, for instance, as we have been discussing under the head of inconstant or simple voltaic batteries. These reversible heat effects indicate the presence of thermal contact-forces which, wherever they exist, prevent chemical data from giving E.M.F. accurately. They also must be taken into account.

chemical but thermal. We have on the one hand a number of Volta series, each for a special medium; and on the other a table of thermo-electric powers at different temperatures. The latter can be conveniently represented by a number of curves, because temperature varies continuously: Volta series, on the other hand, can hardly be represented geometrically, because the transition from one medium to another is probably *per saltum*; at least, it is not known what is the effect of mixing media, and so passing gradually from one to the next.

We have given several Volta series; and, for the sake of completeness, I will now give some Peltier series for a few substances, according to the experiments of Professor Tait, at different centigrade temperatures. Expressing each number as a function of the temperature, we are able to give an infinite number of Peltier series in one table. The range of temperature over which this table may be interpreted is from $-18°$ to $400°$ or so, provided the metals do not begin to melt. Non-metallic substances have not yet been introduced into such series: much experimental work remains to be done before they can be. The metals were not chemically pure.

True Contact E.M.F., or Peltier Series. (Microvolts.)

Metals.	At any temperature $t°$ C.	At 10° C.	At 100° C.
Iron	$-4{,}760-3{\cdot}94t+{\cdot}0487t^2$	− 4,795	− 4,667
Hard Platinum	$-718-{\cdot}54t+{\cdot}0075t^2$	− 722	− 697
Soft Platinum	$+168+3{\cdot}63t+{\cdot}011t^2$	+ 205	+ 641
Magnesium	$-618+{\cdot}36t+{\cdot}0095t^2$	− 613	− 487
German Silver	$+3{,}310+26{\cdot}17t+{\cdot}0512t^2$	+ 3,577	+ 6,439
Cadmium...	$-731-14{\cdot}46t-{\cdot}0429t^2$	− 880	− 2,606
Zinc	$-643-8{\cdot}95t-{\cdot}024t^2$	− 735	− 1,778
Silver	$-590-6{\cdot}26t-{\cdot}015t^2$	− 654	− 1,366
Lead	0	0	0
Copper	$-374-3{\cdot}96t-{\cdot}0095t^2$	− 415	− 865
Tin	$+118-1{\cdot}08t-{\cdot}0055t^2$	+ 107	− 45
Aluminium	$-211-{\cdot}31t-{\cdot}0039t^2$	+ 207	+ 141
Palladium	$+1{,}718+16{\cdot}15t+{\cdot}036t^2$	+ 1,883	+ 3,693
Mercury, C. L. Weber ...	$+1{,}181+5{\cdot}68t+{\cdot}005t^2$	+ 1,238	+ 1,800

To find the E.M.F. of a junction at specified temperature, we

have only to subtract the numbers in the above table, inserting the value of the temperature. Thus a junction of zinc and copper at 10° has an E.M.F. of 320 microvolts, acting from copper to zinc; and a unit current sent across such a junction from copper to zinc, or from zinc to copper, absorbs or generates heat at the rate of 320 microwatts, and the current gains or loses energy at the same rate. Clerk Maxwell says that the force is one microvolt, and that it acts from zinc to copper ("Elementary Electricity," p. 149, *note*); but this is incorrect.*

Hitherto we have supposed the circuit to be all at one temperature; but if different parts are at different temperatures, we shall have to use a yet further series, viz., a Thomson series, for the E.M.F. acting in any one substance with a difference of temperature between its ends, or the force acting at a junction of two pieces of the same metal at different temperatures. This series can be deduced from the preceding, using only the coefficient of t^2, and multiplying it by the difference of the squares of the *absolute* temperature of the two ends of the piece of metal. Such a series then stands thus:—

Thomson Series, or E.M.F. in a metal whose ends are at t_1° centigrade and t_2° centigrade respectively.

(*Microvolts.*)

Iron . .	$\cdot 0487\,(t_1 - t_2)\{274 + \frac{1}{2}(t_1 + t_2)\}$
German silver .	$\cdot 0512\,(t_1 - t_2)\{274 + \frac{1}{2}(t_1 + t_2)\}$
Zinc . .	$-\cdot 024\,(t_1 - t_2)\{274 + \frac{1}{2}(t_1 + t_2)\}$

and so on.

Whether a series of this sort can be made to include any non-metallic conductors also, has not yet been discovered. M. Bouty's experiments provisionally indicate the very interesting fact that Sir

* It is always easy to tell from thermo-electric data which way the force acts at a junction; but it is not always the same way as the current flows, by any means. A current often flows against the E.M.F. even at a hot junction, and it *may* flow against the force at *both* junctions. This is the case, for instance, in a copper-iron circuit with one junction above 275°, and the other below it by a greater amount. It is customary to say that the current flows across a hot junction from the metal of higher, to the metal of lower, thermo-electric value: this is not necessarily true. The safe statement is to say that the force acts from high *to low thermo-electric* value at either junction.

W. Thomson's general thermo-dynamic laws of the thermo-electric circuit apply perfectly to circuits which include some electrolytes as well as metals.

Now the meaning of statement No. xxiv. is as follows:—Regard zinc and copper as a circuit completed by air or by water, as the case may be, and let the temperature be uniform, and say 10°; to reckon up the total E.M.F. we must look in the proper Volta series for Zn/air (or Zn/water), which we find 1·8, say; for Zn/Cu, which we don't find, or find zero; for Cu/air, which we find ·8. Then we must look in the 10° Peltier series for Zn/air or Zn/water, which at present we shall not find there for want of data (possibly we have no right to put them there if we had data); for Zn/Cu, which we find about 320 microvolts; and for Cu/air, which again we don't find. Add them all up with their proper signs, and we have the total E.M.F. of the circuit.

Again, consider the case of a Daniell cell at a given temperature producing a current. We shall have to look out in each series for $Zn/Zn,SO_4$, for $Zn,SO_4/Cu,SO_4$, for $Cu,SO_4/Cu$, and for Cu/Zn, and add them all up. It is true that these tables of numbers have practically yet to be made, for at present they include so few substances: that does not affect the question of the existence and independence of these two kinds of series.

It is, of course, a question how far *all* E.M.F. of contact may be found to depend on chemical tendency. For instance, when bismuth and antimony are put into contact, does the E.M.F. developed measure the alloying affinity of these two metals? When sodium is dropped into mercury, does the heat produced represent the thermo-electric power of a sodium-mercury junction? When metal touches glass, does the tremendous E.M.F. developed represent a tendency of the metal to combine with the glass? These are questions for experiment to decide; but to me it does not seem probable that it will reply in the affirmative.

We know that Sir W. Thomson, and Davy before him, considered the apparent contact-force at the junction of zinc and copper to be due to the chemical affinity between these two metals, and to be measured by the heat of formation of brass; *but this we have* seen strong reasons for disbelieving. It sounds

more probable that the real contact-force at a junction of bismuth and antimony should be due to the chemical affinity between these metals; but I believe it is no more true. I regard the greater part of a contact-force of this kind as due to a physical difference between the metals, such as a difference in atomic velocity, and to have no close relation to their chemical affinities for each other. It may, however, well be that *some part* of an observed junction force is due to this chemical tendency between two metals. For instance, take the case of zinc and copper. There is, I suppose, an undoubted affinity between them, as shown by the formation of brass under proper conditions. [If chemists assume the right to demur to this on the ground that the two metals mix equally well in any proportions, one can choose any other pair of metals—say, perhaps, copper and tin—for which this statement does not hold.] Now, does this affinity result in any E.M.F. between them on making contact? This question, I apprehend, is to be answered by passing a current for a long time across a copper-zinc junction and seeing if any brass does, after a long time, result. Thermopiles show a curious secular deterioration with use, and it may be that some alloying action goes on, though I have never heard of its being noticed. But if no such alloying goes on during the passage of a current, then I should say that, in whatever ways chemical affinity between two metals is able to show itself, it does not show itself as an E.M.F.

Observe, I do not for a moment question the existence of a few hundred microvolts of E.M.F. at a zinc-copper junction. I only ask, Is this chemical? or is it physical? or is it a mixture of the two? Statement No. xxiv. is general enough to take into account the possibility of its being a mixture of the two. It is easy to write one of them zero, if so it turns out.

15. The remainder of my notes aim at summarising, in a compact form, the main argument with respect to the immediate subject of discussion, viz., the seat of electro-motive force in a voltaic cell, and in ordinary Volta condenser experiments.

IV.—Brief Summary of the Argument.

xxv. Wherever a current gains or loses energy, *there* must be a seat of E.M.F.; and conversely, wherever there is a seat of E.M.F., a current must lose or gain energy in passing it.

xxvi. A current gains no appreciable energy in crossing from copper to zinc, hence there is no appreciable E.M.F. there.

xxvii. Where a current flows from zinc to acid, the energy of the combination which occurs is by no means accounted for by the heat there generated, and the balance is gained by the current; hence at a zinc acid junction there must be a considerable E.M.F. (say, at a maximum, 2·3 volts).

xxviii. A piece of zinc immersed in acid is therefore at a lower potential than the acid, though how much lower it is impossible *precisely* to say, because no actual chemical action occurs. [If chemical action does occur, it is due to impurities, or at any rate to local currents, and is of the nature of a disturbance.]

xxix. A piece of zinc, half in air and half in water, causes no great difference of potential between the air and the water (Thomson, Clifton, Ayrton and Perry, etc.), consequently air must behave much like water.

xxx. If it makes the air slightly positive to the water, as it does (Hankel), it may mean that the potential energy of combination of air with zinc is slightly greater than that of water, or it may represent a difference in the thermo-electric contact-forces between zinc and air and zinc and water, or it may depend on a contact-force between air and water. [If such a contact-force between air and water exists, it is obviously of great importance in the theory of atmospheric electricity, for the continual slow sinking of mist globules through the air would render them electrical.]*

xxxi. Condenser methods of investigating contact-force no more avoid the necessity for unknown contacts than do straightforward electrometer or galvanometer methods; the circuit is completed by air in the one case and by metal in the other, and

* *Cf.* Lecture on "Dust," Nature, Jan. 22, 1885.

the E.M.F. of an air-contact is more hopelessly unknown than that of a metal-contact.

xxxi. All electrostatic determinations of contact-force are really determinations of the sum of at least three such forces, none of which are knowable separately by this means.

xxxiii. The only direct way of investigating contact-force is by the Peltier effect or its analogues. [Maxwell.]

xxxiv. Zinc and copper in contact are oppositely charged, but are not at very different potentials; they were at different potentials before contact, but the contact has nearly equalised them.

xxxv. The potential of the medium surrounding them is, however, not uniform. If a dielectric, it is in a state of strain; if an electrolyte, it is conveying a current.

The President: You will all, I am sure, agree with me that Professor Oliver Lodge has given us a most excellent paper. It is past our usual time to commence a discussion, but as Professor Fitzgerald, of Trinity College, Dublin, is here, and cannot attend next time, and as we shall be very pleased to hear any remarks from him, I will ask him to kindly open the discussion, which will then be adjourned until our next meeting.

Professor Fitzgerald: I may say that on the whole I entirely agree with Dr. Lodge in his position—that is, with the actual position he has taken up—as I think it explains what most likely takes place; but at the same time I do not agree with his opinion that his position is proved by actual experiments. There is one point he has raised which, if his statement were actually the full statement of what must take place, would, I think, perfect his position; and it is a position which Maxwell took up, and therefore is deserving of a great deal of consideration. But I do not agree with him in thinking that the Peltier effect is necessarily the same thing as the whole electro-motive force between two metals. He has given an illustration which, however, I do not think represents what may be the state of affairs. I want you to observe my position—that up to the present the whole question is undecided; that *there are no experiments* that are able to decide

where the seat of electro-motive force is, and it is quite possible that, as Mr. Gibbs said in Montreal, the question is one which is undecidable; and that when we know really what electro-motive force is we may not have to ask *where* it is, because it may be everywhere—in the whole of space, for the matter of that. I will illustrate the difference between what I consider may be the true explanation of what takes place in the contact of the two metals and the connection it may have with the Peltier effect. Now, the analogy was between two vessels representing zinc and copper, placed beside one another. In that case there is in their position nothing to cause a difference of pressure between the two. To illustrate any attraction between the zinc for electricity and copper for electricity I would rather place the two vessels, the zinc vessel below, and the copper vessel above: that might represent the state of affairs in which there was a greater electric pressure in one than in the other vessel—that difference of fluid pressure being produced by the gravitation of the earth, and, in the case of the zinc and copper, the difference of electric pressure by the attraction of the zinc for the electricity and copper for the electricity. Now, if there is electricity flowing in this circuit, the potential of the electricity will continually rise up from the zinc to the copper and fall down again, but it would not produce any thermal effect. Now, in order to represent the Peltier effect, we might add at the junctions of zinc and copper little paddle-wheels, such as Professor Lodge supposed produced the whole effect; and one of these turning when the current runs might do work in winding up a weight, and so represent the heat developed at that junction, while the other unwinding would represent the heat absorbed at the other junction. This action would only take place while the current was running. The Peltier effect *may* thus be quite a different thing from the difference of potential produced by contact. On the Peltier effect depends the rate of variation of potential difference with temperature, but not the difference of potential itself. This latter *may* be produced by a difference of attraction of zinc and copper for electricity. I do not think Maxwell was right in saying that *the heat developed* in the Peltier effect *necessarily* measures the

difference of potential in different metals. Professor Lodge has been misled by his analogy.

2nd April, 1885.—*Note added since discussion.*—The question hinges very much on the definition of "potential of the metal." If by this is meant the external work that can be done by electricity flowing from the metal to some metal at standard zero potential, then the Peltier effect by definition measures the potential of the metal. But that may not represent everything that takes place. The metal may produce potential in a vacuum in its neighbourhood, and so we may define the potential of a metal to be the same as that of the vacuum in its neighbourhood; and this may be quite a different value from that derived from the former definition. When water flows in a pipe up and down, its power of doing work is the same at all levels, but its pressure and the strain it produces in the pipe are not. Its power of doing work corresponds to the first definition of potential, its pressure and the resultant strain to the second.—GEO. FRAS. FITZGERALD.

The further discussion on the preceding paper was adjourned until Thursday, 23rd April.

A ballot then took place, at which the following were elected:—

Associates:

Mads Peter Hardt.	Henry Jackson.
Walter T. Goolden.	John Alexander McMullen.

Lieut. W. Clifton Slater, R.N.

Students:

George H. Nisbett.	Frederick Paton.

The One Hundred and Forty-fourth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, 23rd April, 1885—C. E. Spagnoletti, Esq., M. Inst. C.E., &c., President, in the Chair.

The Secretary read the minutes of the last meeting.

Donations to the Library were announced as having been received since the last meeting from the following:—Latimer Clark, Muirhead, & Co.; F. Cardarelli, Foreign Member; J. M. Collette, Foreign Member; O. Schäffler, Foreign Member; W. T. Ansell, Member; W. H. Preece, Past-President; Sir David Salomons, Bart., Member; C. Todd, Member.

A hearty vote of thanks was accorded to the donors.

The adjourned discussion of Professor Oliver Lodge's paper was, at the invitation of the President, resumed by Professor Perry.

Professor Perry: Dr. Lodge's paper differs from those usually read at this Society in that it opens a debate; and my answer can hardly be regarded as merely a criticism of an ordinary scientific paper, but as opening the debate on the other side. Dr. Lodge is the exponent of the views which have been held by Clerk Maxwell and other philosophers, and I want to put before the Society the way in which, with Professor Ayrton, I have looked at the question from what may be called the Thomson side. Like almost everybody else we use the expression "difference of potentials" between two points as meaning, in electrokinetics, the electrostatic difference of potentials between two copper wires coming from the points. This practice is very convenient, and there can be no objection to it so long as we do not imply that it is always the same as the electrostatic difference of potentials between the points. It is in this that Dr. Lodge differs from us: he says, that this is the electrostatic difference of potentials, and I am compelled to think only of electrostatic difference of potentials in this discussion. *My strongest* feeling just now is one of gratitude to Dr.

Lodge; he has treated us mercifully on the whole. When one mounts the steed of physico-chemical speculation as to what is going on in a voltaic circuit there is no reason for his drawing rein at one place rather than another. Why should he stop anywhere in particular? It is only enjoyment to him this careering about in a new world where there is neither north nor south, where one plain fact is an enchanted giant error that must be borne down by the spear of downright denial, and another plain fact is simply a plain fact with no enchantment about it. I say again that I feel grateful to Dr. Lodge, because there is no doubt that he has the power of speculating for years and years on a subject of such interest as this. He accepts the contact theory of voltaic action; he acknowledges that the electro-motive force of a voltaic circuit is equal to the sum of the contact-forces, as measured electrostatically, of the various substances. These are the plain facts; now for the enchantment. He says it is absurd to imagine that there is a considerable difference of potential between two metals, say zinc and copper, in contact with one another. He says, "Yes; experimenters have certainly found an apparent difference of potential; they have measured apparent differences of potential in air, and they are due to air effect." Yet they have measured these differences of potential, when, instead of air being the dielectric, there has been no air, and there have been other gases quite different from air, but still he says their results are due to an air effect. They have even measured these differences of potential in fairly good vacuo, but still Dr. Lodge says they are air effects. Not only does he say that this is the case, but that if you could experiment in as perfect a vacuum as has been obtained by Mr. Crookes and obtained the same result, as we suppose you might, still he says it would be due to an air effect. Now, when Dr. Lodge makes a statement of that kind, it is quite obvious that there must be some very strong reason for his discarding evidence; for you must remember that there is no rebutting evidence, that in Brown's experiments there really were salts formed on the metals, and we are all agreed as to what effects such salts *produce. The* strong reason causing Dr. Lodge

to discard evidence in this way, this strong reason—whatever it may be—has caused him to enter into elaborate speculations as to the attractions between gases and metals, and I assume that if we can show Dr. Lodge that this strong reason is baseless—has no existence—then he will probably be willing to leave again this region of speculation to the sole enjoyment of the leader from whom he already differs a little, Dr. Exner.

Now, first let me dispose of the part of his paper which has nothing to do with the debate, namely, the seat of the electromotive force in a voltaic circuit. With regard to this matter, we are, I believe, in agreement, and I hope to prove that this seat of the E.M.F. in a voltaic circuit has really nothing whatever to do with difference of potential between zinc and copper in contact; and, in fact, that Dr. Lodge's paper has not got quite the proper title. His paper is really devoted to quite a different subject from that of the seat of E.M.F. in a voltaic cell. I shall regard the Peltier effect as non-existent, merely for the sake of shortening my statements. Any cause of flow of electricity used to be called an E.M.F., and hence, in stating Ohm's law, E.M.F. and difference of potential were used indifferently. But when we adopt, as everybody does nowadays, the exact definition of Clerk Maxwell, we must be more careful; and when we adopt the much more guarded definition of E.M.F. of Dr. Lodge, we must be particularly careful. This definition of Dr. Lodge is given in two quotations from Clerk Maxwell; and the definition is not merely the definition of E.M.F., but is the definition of the seat of the E.M.F. The seat of the E.M.F. in any circuit is the place at which the circuit receives or gives up energy as distinct from heat due to resistance, and the amount of the E.M.F. is measured as the amount of energy which enters the circuit per unit of electricity passing that place. Now, how is it possible, with a definition of E.M.F. like that, to give an answer to Dr. Lodge's statement different from what he has given? The seat of E.M.F. is practically defined to be the place where the chemical action goes on. It follows that the place where the chemical action goes on is the seat of the E.M.F. Even a logical machine would *say that this* answer follows from the definition, and the definition

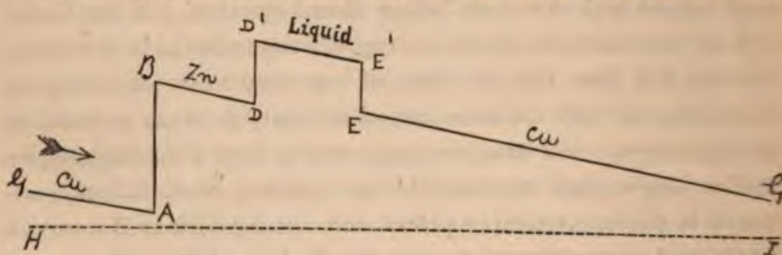
is merely a partial statement of Sir William Thomson's result—that the E.M.F. of a voltaic circuit can be calculated from the chemical action going on.

But Dr. Lodge's paper takes up a very different matter. He finds strong reasons for believing that there cannot be a considerable difference of potential between zinc and copper in contact. In fact, the difference of potential is to him logically absurd, and he devotes the paper to speculations as to how experiments could have misled Sir William Thomson and others into believing such an absurdity. The absurdity is so obvious to Dr. Lodge, that it might have struck him, perhaps, that Sir William Thomson might also have seen it. However, Dr. Lodge has the opinions of Clerk Maxwell and many others on his side, and in fact this is the subject of debate. I shall endeavour to show that our notion does not lead to such an absurdity, and hence that Dr. Lodge's speculations, however interesting, are unnecessary. With his definition of E.M.F. we have never stated that there is such an E.M.F. due to contact between zinc and copper. We deny that there is such contact E.M.F., but we do say there is contact difference of potential. We do not say that what has been measured may not be to some, even to a large, extent due to air effect, but we contend that no such air effect has ever been observed, and until it has been observed we cannot admit that there is any strong reason for believing in its existence. Dr. Lodge says, if the electricity rises in potential from copper to zinc there must be a supply of energy from some outside source at the place of contact. Now, we ask him, why? We do not ask him what he means when he says that electricity rises in potential at the point, or what he means by potential inside a wire in which a current is flowing, or whether he is speaking of electrostatic-potential, or why he assumes that there is the same or nearly the same potential at different points of any cross-section of a wire. He cannot answer these questions unless he knows what is the distribution of electricity through the copper mass. Now, considering that we do not know whether it is a trillionth of a unit of electricity per cubic millimètre, or whether *there are* trillions of units of electricity per

cubic millimètre; considering that we do not know whether there is any electricity at all in the body of the wire, we believe that these questions stand in the way of any logical proof such as Dr. Lodge supposes he can give of the absurdity he is speaking of. Well, we will not stop him by asking him any of these questions. In fact we are willing to assume that, in taking a unit of electricity from a point in a section of the wire to the outside and from that through the dielectric, and from that into the wire to a point in another section, the work that must be done is the same for any points in the sections as Dr. Lodge assumes, and to take that work as the measure of the difference of potential between these two sections; and still we ask him why we must assume that, when electricity rises in potential suddenly at a place in a circuit, energy must come in from the outside to enable the electricity to rise in potential. Suppose we grant that electricity gains in potential-energy in flowing from copper to zinc, do we know so much of an electric current as to assert that when the electricity gains potential-energy it does not lose an equivalent quantity of some other kind of energy? Why may we not assume, for example, that electricity loses tension-energy in passing from copper to zinc, so that if it gets potential-energy it loses tension-energy. From that point of view, part of the function of the liquid in the cell would be taking electricity from the zinc to the copper inside the cell, giving it tension-energy, and this tension-energy is converted into potential-energy when the electricity passes in the outer part of the circuit from copper to zinc. But, indeed, there is no need to assume the existence of a tension-energy. Why should we assume that work must be done by outside agents at a place in raising electricity from potential A to potential B? By definition work will be done if we take electricity through the dielectric from A to B, but it does not follow that an outside source must do work in carrying electricity from A to B through the wire itself. We know so little of what is really meant by the electric current, and the mechanical analogies which have been put forward from time to time are so imperfect, not even excluding the beautiful form that Mr. Poynting has given to Clerk Maxwell's analogy,

that I always like to fall back upon the oldest and simplest one; and the oldest and simplest one is this: When electricity is flowing in a copper wire there is electricity in every part of the wire and not merely on the outside of it. Now at one end—the battery end—of this copper wire, electricity is continually being thrust in, and in consequence at the other end, where the copper joins the zinc, there is a tendency for electricity to leave the copper and enter the zinc, just for the same reason that electricity tries to come to the outside of bodies—repulsion of electricity for itself. If at the zinc end, electricity can only get away by rising in potential, then it rises in potential, but the energy which there becomes potential-energy comes from the battery. It is not necessary to get a supply of energy from an outside source at the junction. We merely put this analogy before the Society for the purpose of giving a clear idea as to how a difference of potential between zinc and copper can be explained without assuming a supply of energy from an outside source at the junction. But after all it is an analogy. When we speak of electricity flowing in a wire, we are using an idea which is derived from electrostatic and hydraulic phenomena to explain a phenomenon of an electrokinetic kind. Our only excuse is that everybody uses some kind of analogy, and ours agrees with the facts better than any other. If Dr. Lodge objects to the idea of tension-energy, or even to the idea of tension, we can tell him that in spite of our very exact knowledge of mechanics we have the same sort of difficulty in many mechanical arrangements. For example, suppose you have a winding-engine and a long endless inextensible rope passing over pulleys, horizontal in places, vertical in places, and coming back to the engine again, and at one place there is a weight being lifted. Now, work is being done on that weight in lifting it. But energy does not come from the outside into the circuit there; it comes from the winding-engine; it comes somehow through the rope. Energy disappears at the winding-engine and appears as work done in the weight being lifted. In what form is it before it is converted into potential-energy? The energy is conveyed. In what form is it when it is *in the rope?* We really do not know, or, rather,

we have not given a name to it. But if we must give a name to it, it is tension-energy. We know that there is a difference in the tension of the rope above and below the weight which is being lifted. Difficulties of the kind I speak of—such difficulties as will tempt Dr. Lodge to prevent our speaking of tension-energy—are really due to the fact that the notion of conveyance of energy from one point to another has not yet been co-ordinated with the ordinary exact definition of energy. The energy which we usually speak of as being possessed by a portion of matter, or as existing at a particular place, is really possessed by a much larger system. But the analogy which I like best, and which in every particular satisfies the voltaic circuit, is the hydraulic analogy. It gives me pleasure to consider it: Firstly, because many who listen to me are in the habit of thinking that electricity passes along a wire as water flows along a pipe; and, secondly, because Dr. Lodge has used this analogy for the purpose of showing the absurdity of our position. In fact we will now give the explanation mentioned by Dr. Lodge in the paper as having come through one of us from Sir William Thomson, but which must really be regarded as having come from ourselves, as we think that Sir William has probably his own distinct method of looking at the matter.



Dr. Lodge has slightly misapprehended it. Suppose we have a circuit of pipes, G A B D D¹ E¹ E G, whose development is shown in the figure, the two ends of the figure G and G being the same point in the real circuit; the pipes to be of uniform section, and to rise suddenly in level from A to B and from D to D¹, and to fall from E¹ to E, and there being a uniform *rate of fall* from B to D, D¹ to E¹, and from E through G to A.

H I may be any datum level. Let us suppose that there is a continuously acting pump in the part $D^1 E^1$, causing incompressible fluid, such as water, filling the pipes to flow continuously. Mechanical energy is given to the pump from some outside source, and the energy reappears at various places in the circuit. Thus, for example, between A and B the water rises in level. A pound of water at B has more energy than a pound of water at A, and its increase of energy is h foot pounds, if the difference of level is h feet between B and A. It has really gained energy. What has it lost? Pressure, certainly; but pressure is not a form of energy. We know that in a steady frictionless stream

$$\frac{v^2}{2\,g} + 2{\cdot}3\,p + h$$

remains constant, if v is the velocity in feet per second, and p the pressure in pounds per square inch, and h height in feet above datum level. As $\frac{v^2}{2\,g}$ is the kinetic-energy of a pound of water in foot pounds, and h is its potential-energy, one is tempted to call $2{\cdot}3\,p$ also a form of energy—pressure-energy, but we have just as much and no more right to call it so than to speak as I did of tension-energy in electricity. We can see that pressure conditions are not in themselves a source of energy, by imagining incompressible fluid, like water, filling a closed vessel at great pressure, and then imagining an orifice to be made, when, of course, the water is no longer subjected to great pressure. But $2{\cdot}3\,p$ enters into the expression and is familiarly spoken of as a form of energy when the stream is *steady*—when there is a continuously acting pump somewhere in the circuit. In fact, then, we see that when a pound of water rises in level from A to B it receives h foot-pounds of energy if h is the difference in level, and it receives no energy from an outside source at A B; energy disappears in the pump, and work is done between A and B. Now, just as difference of potential in electrical things is measured as the work which must be done, on unit of electricity, in taking it from one point A to another B through the dielectric, so the difference of level between A and B represents the work done on a pound of water *in taking* it from A to B through the air.

I will not trouble the meeting by stating exactly how a pound of incompressible fluid may be taken from A to B through the air, having *h* foot-pounds of work done upon it, but anyone who has studied hydro-dynamics sees at once how it may be done. Now, this is not merely an analogy; it really represents the true state of things in a voltaic circuit. There is a tendency for electricity to leave copper and enter zinc at the junction. We are prevented from calling this the E.M.F. of contact, so we will call it contact-force. It is measured as the difference of potential established between two metals which are in contact. There is no contact-force in a piece of copper or in a piece of zinc. The contact-force is analogous with pressure in water. There is a sudden rise in level in the pipe from A to B which corresponds with a sudden rise of potential from copper to zinc. There is a consequent diminution of pressure from A to B which corresponds with the contact-force at the junction. There is no contact-force in a copper wire, and hence it is necessary to give to the pipe such an inclination that the potential-energy lost by the water in flowing along the pipe shall be equal to the energy given out in friction. In the voltaic circuit, similarly, the fall of potential-energy is known to be equal to the heat produced in overcoming resistance.

Should the external circuit consist of several wires of zinc or other metal and copper, it is easy to draw the development of the pipe-circuit analogy. Supposing the water to be slightly compressible, then maintaining any vertical parts of the pipe at different constant temperatures, will help or oppose the action of the pump in the circuit, so that we have thermo-electric effects represented. Thus, not using a pump, let there be an endless pipe, rising suddenly from A to B, horizontal from B to C, falling suddenly from C to D, and horizontal from D to A. Imagine this filled with a slightly compressible fluid, and that the limb C D is permanently kept at a higher temperature than A B, there will be a steady flow maintained in the direction A D C B A. If the limb A B is the higher in temperature, the flow will be in the direction A B C D A. Strictly speaking, we ought to consider the fluid in the endless tube to be one which changes the law

connecting its density and temperature at a critical point, like water at 4° C.*

To sum up, then, if we define (why it is necessary to do so we know not) the seat of E.M.F. as the place where energy disappears in a voltaic circuit, and the amount of this E.M.F. as the amount of energy disappearing per unit of electricity flowing in the circuit, then it is obvious that the seat of the E.M.F. of a voltaic circuit (neglecting Peltier effect) is at the place where chemical action occurs. There is no seat of E.M.F. at the junction of zinc and copper, but there may be a considerable difference of potential between zinc and copper. That is, to carry unit of electricity from the surface of the copper to that of the zinc, through the dielectric (even if this dielectric is the interstellar ether), an outside source must do work upon it, but it does not follow that an outside source must do work upon electricity flowing in the copper to make it pass into the zinc. This is exactly analogous with water when flowing in pipes which rise suddenly in level from A to B. To carry a pound of water, regarded as incompressible, from A to B, through the air, the energy required is h foot-pounds for a difference of level of h feet. But when a pound of water passes from A to B by the pipe in which it is steadily flowing no work needs to be done upon it by outside agencies; the force acting on it at any instant is $2{\cdot}3\,\frac{dp}{dx}$, and the integral $\int 2{\cdot}3\,\frac{dp}{dx}\,dx$

* *Note added May 15th.*—To be quite correct, let what I have called "tension" in a metal a, at the absolute temperature t, be denoted by ${}_t\mathrm{P}_a$; and the increase of potential from a to a metal b, as measured inductively, by ${}_t\mathrm{V}_{ab}$, then the electro-motive force of a thermo-electric circuit of two metals, a and b, whose junctions are at temperatures t_1 and t_2, is

$$\mathrm{E} = {}_{t_2}\mathrm{V}_{ab} - {}_{t_1}\mathrm{V}_{ab}.$$

The Peltier effect at a junction is

$${}_t\Pi_{ab} = {}_t\mathrm{P}_b - {}_t\mathrm{P}_a - {}_t\mathrm{V}_{ab},$$

where

$${}_t\mathrm{V}_{ab} = (k_b - k_a)\,t\,\{\mathrm{T}_{ab} - \tfrac{1}{2}\,t\} + \mathrm{C}_b - \mathrm{C}_a$$

$${}_t\mathrm{P}_a = \tfrac{1}{2}\,k_a t^2 + \mathrm{C}_a.$$

k_a and C_a are constants peculiar to the metal a, and T_{ab} is a constant peculiar to the two metals a and b. Hence ${}_t\Pi_{ab} = t\,.\frac{d}{dt}.\,{}_t\mathrm{V}_{ab}$, or the Peltier effect, is proportional to the absolute temperature and to the rate of change of the contact-force *with temperature.*

is equal to h. If we care to think of the potential-energy gained by the water, we may say that this has come from the pump, and we know all about the agency by which it has come; but if we are forced to consider what is the form of the energy of a pound of water immediately before its conversion into potential-energy h, we are compelled to say "pressure-energy," represented by 2·3 times the difference of pressure.

Dr. Lodge may, by defining potential in a new way not recognised in electrostatics, prove that there can be no difference of such new kind of potential between zinc and copper in contact; it is my opinion that this is what he and Mr. Poynting and Clerk Maxwell and many others have unconsciously done.

Added May 5th.—Dr. Lodge, in his reply to remarks, was kind enough to mention the fact that my colleague and I had drawn Mr. Poynting's equipotential surfaces and lines of flow of energy as they must be drawn in practice, most of them lying like successive sleeves in an infinitely shallow space round all the conductors in a voltaic circuit, if we are to follow Maxwell in assuming a considerable difference of potential between air and metals. As Dr. Lodge has stated, we have also drawn these surfaces and flow of energy lines on our own assumption of no air effect and considerable contact-differences of potential. Dr. Lodge has not, however, referred to the very much more interesting curves which Prof. Ayrton and I have drawn as the energy lines from a primary voltaic or thermo-electric circuit whose current is altering just in such a way as to keep the current in a secondary circuit constant. Suppose it is the E.M.F. of the primary which is altering, it is very interesting to see how the lines of flow of energy sweep over the primary circuit, their parts which cut the secondary remaining unaltered. But now, suppose the total E.M.F. of the primary to be less than one volt, and imagine that Prof. C. Maxwell and Prof. Poynting and Dr. Lodge were right in their idea of a considerable difference of potential between air and metals, it will be found that no line of flow of energy can connect the primary and secondary circuits, since all the equipotential surfaces of the primary will lie infinitely close to the conductors of which it is *composed*, and yet it cannot be doubted that the secondary circuit

receives energy from the primary. If I am wrong in this I hope that Mr. Poynting will correct me; but it seems to me that Maxwell's electro-magnetic theory actually requires us to deny the existence of a considerable air effect such as Maxwell himself insisted on, if we are to make it consistent with observed phenomena. J. P.

Dr. J. HOPKINSON: I am rather disposed to agree with Professor Perry thus far—that this question is one very largely of definition. Professor Perry defines the difference of potential between a piece of copper and a piece of zinc to be the energy required to transfer a unit of electricity from near the copper to near the zinc through the dielectric. On the other hand, as I understand the matter, Dr. Lodge, and probably Clerk Maxwell, define the difference of potential to be the energy required to transfer a current of electricity from the copper to the zinc through the junction between the copper and the zinc. Now, it appears to me that Dr. Lodge has very clearly shown that his way of looking at the matter is the more convenient; for, in the first place, it expresses, with the greatest ease, all facts that have been observed. When copper and zinc are in contact with air there is a difference of potential at the three junctions, and the sum of the three differences of potential is not equal to zero; and we have no difficulty in explaining all the electrostatic effects that we have obtained. Dr. Lodge has so clearly proved the convenience of his way of looking at the matter that it would be only wasting your time if I were to attempt to reproduce his arguments. It seems to me better to take that basis and pursue it. Now, if we take that basis as our starting point, it is very easy to set forth all the theory of thermo-electricity, and the formulæ given at the end of Clerk Maxwell's chapter on that subject can be proved with the greatest ease; it is also easy to extend these formulæ to the case in which we have chemical action going on, and to foreshadow the precise method by which we may be able to ascertain exactly what the difference of potential is between copper or zinc or other metals and electrolytes. I will not take up your time by going over that ground

now, but I will pass on to another point, which I think has not been dealt with by Dr. Lodge, and it is this:—Take the case of a Daniell cell, consisting of copper, sulphate of copper, sulphate of zinc, and zinc. I do not think Dr. Lodge makes clear whether the difference of potential is between the copper and the sulphate of copper and between the zinc and the sulphate of zinc, or between the sulphate of copper and sulphate of zinc. Now that question is identical with that of whether, in the case of electrolysing sulphate of copper with two copper terminals, there is a large difference of potential between copper and sulphate of copper at each end or whether there is not. Now, I think we have sufficient experimental knowledge to settle that question off-hand—it is closely connected with the observation which is familiar to every one that, if you are electrolysing sulphate of copper, where copper is deposited you get an impoverishment of the solution, whereas at the other terminal, where the copper is being dissolved, you get a concentration. From that observation it follows, I think, beyond all doubt, that the difference of potential is considerable between the solution and the metal and is small between the sulphate of zinc and the sulphate of copper.

Dr. Fleming: I do not know that at this stage I can contribute very much to the debate. The principal points in question have been dealt with exhaustively already. I must confess I cannot follow very well Professor Perry through his rather intricate reasoning, but my own position coincides very closely with that of Dr. Lodge.

If anything could possibly add to the great value of the *résumé* of this subject, which Dr. Lodge has presented to us, it would be if he could have pointed out to us the best direction for research needed to clear up points still in dispute. It seems quite clear that no experiments on contact-electricity in a so-called vacuum are of any use, on account of the impossibility of removing more than a fraction of the atmosphere and of the adhereing gas films. Also, with regard to Mr. Brown's experiments, made in sulphuretted atmospheres, no positive conclusions can be drawn on account of the introduction of films of oxides and sulphides, *&c.; but it* may, perhaps, be possible to make experiments on

carefully cleaned metals immersed wholly in a neutral non-conducting liquid, such as paraffin, and endeavour to ascertain if the phenomena are affected.

Experiments of this description might be worth trying again, very carefully, and might possibly contribute to clear up the question of the dependence of contact-electricity on the surrounding medium.

Professor GEORGE FORBES: I will say very few words, and these mostly not my own. The question has little more interest to me than such metaphysical speculations as "How many angels can stand on the point of a needle?" We cannot arrive at any conclusion in the matter without experiment. Dr. Lodge has shown reason for doubting the conclusions of all old experiments, and he has given no new ones. I expect he will say, in answer to Dr. Fleming, that no experiment would be conclusive to him. Either view is sufficient as a working hypothesis, and each person may use as such whichever view seems to him simplest. Dr. Hopkinson thinks Dr. Lodge's way of putting it is the simpler. I think the other is. And so we agree to differ. My only reason for rising is to read a few remarks from Sir William Thomson, on the Peltier effect, in a letter I received to-day. He there says—

"The existence of the Peltier effect proves, not simply difference of potentials between two metals in contact, but variation of this difference with temperature. Thus, if the contact-difference of potentials were the same at all temperatures there would be no Peltier effect.

"At temperatures for which the contact-difference between two metals is a maximum or minimum (for example, 284° C. for copper and iron), the Peltier effect vanishes, but the contact-difference of potentials is not *nil*.

"The invocation of the Peltier effect is not particularly happy or instructive in connection with Volta's contact-difference of potentials. Consideration of the mere facts of thermo-electricity, without specially thinking of the thermo-dynamically related Peltier effect, is simpler in respect to voltaic theory.

"Variation of difference of potentials between two metals, with variation of *temperature*, is the fundamental phenomenon of

thermo-electricity, but no observations upon thermo-electric currents in closed circuits can give us any idea of the absolute value of the difference of potentials of which they prove variation with temperature. The smallness of this variation (amounting, even in so large a case as that of copper and bismuth, to only about $\frac{1}{180}$ of a volt for 100° C. difference of temperature) is remarkable in comparison with the largeness of the absolute difference of potentials (amounting, for example, in the case of copper and zinc, to about ·7 of a volt). The most delicate measurements of Volta-contact-difference, which I have myself made, are sensible to about $\frac{1}{200}$ of a volt, and I think it would not be difficult to attain 10-fold greater sensibility. But attempt to measure, electrostatically, the change of Volta-difference of potentials with change of temperature would probably be baulked by the surface changes (formations of films of oxide, or chloride, or iodide, or possible changes of amount of attached films of gases not chemically combined according to ordinary ideas) which are inevitable with changes of temperature, unless the metallic surfaces electrically tested are protected from such influences by being in a sprengel vacuum."

Professor Ayrton: With reference to the interesting remarks of Sir William Thomson that Professor Forbes has kindly communicated to us, on measuring what may be called the contact-potential difference of metals at different temperatures as distinct from the Peltier effect, I may mention that my colleague, Professor Perry, and I carried out in 1878 a number of experiments on this very subject; and as these experiments have not yet been described, nor the results published, and as, in addition, they illustrate in a very forcible manner the difficulties Sir William has anticipated would probably be introduced by the formation of films of oxide, I may perhaps be permitted to say a few words about them this evening.

On the last occasion Dr. Lodge explained the general construction and mode of using the apparatus we employed in Japan for measuring the contact-potential difference of solids with solids, solids with liquids, and liquids with liquids, so that you will understand, without further description on my part, how, by means of that induction apparatus, we were able to measure the

contact-potential difference of hot and cold mercury. The hot and the cold mercury were put into two porcelain dishes side by side, which had been thoroughly de-electrified by having been passed through the flame of a spirit lamp, and the two mercuries were connected by a small column of mercury in a glass syphon tube. The mercury in one of the vessels was electrically joined to the metal case covering the whole apparatus, since* this arrangement diminishing the error arising from any want of perfect symmetry in the apparatus.

The following are samples of the results obtained:—

April 1st, 1878.

Temperature of Hot Mercury.	Temperature of Cold Mercury.	Potential Difference in Volts.
48°C.	17°C.	0·97
35°	,,	1·00
32°	,,	0·89
27°	17°·25	0·87
22°·5	,,	0·70
20°	,,	0·53
18°	17°·5	0·36
17°·8	17°·5	0·20
17°·5	17°·5	0·14

April 4th, 1878.

53°	16°	1·19
46°	,,	0·95 (Varied from 0·88 to 0·97 in different observations.)
40°	17°	0·94 (Varied from 0·91 to 0·95.)
31°	18°·8	0·82
23°·5	17°	0·66

At this period, the gilt induction plates were found to be somewhat affected by the mercury vapour, and the experiments were stopped until the plates had been regilt.

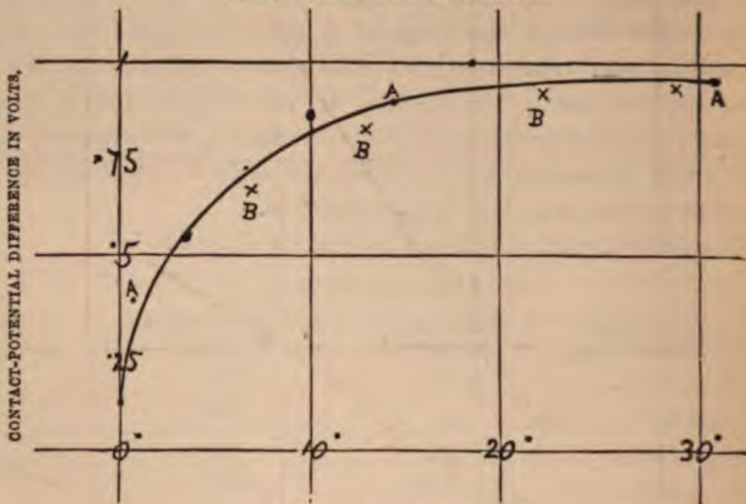
In all cases the cold mercury was positive to the hot.

If the above results are tabulated for difference of temperature and plotted, we obtain two sets of points, A A A and B B B,

* "On the Contact Theory of Voltaic Action." Paper No. III., *Phil. Trans.*, Part I., 1880.

shown respectively by dots and crosses, and which lie roughly on the curve shown.

HOT AND COLD MERCURY.

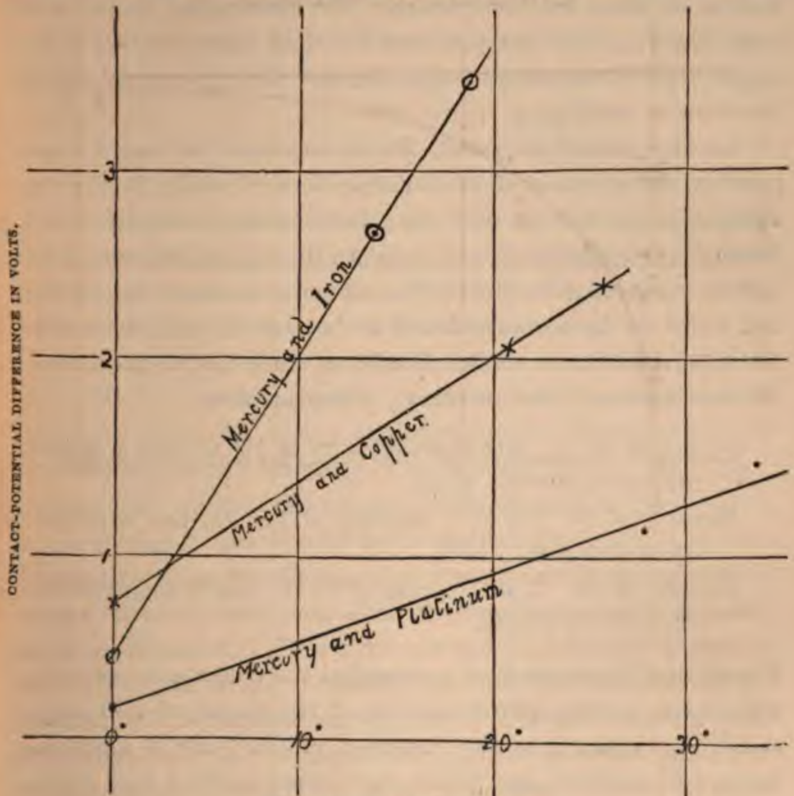


DIFFERENCE OF TEMPERATURE.

Another set of experiments was made to determine the contact-potential difference of mercury with iron, mercury with copper, mercury with platinum, and mercury with lead, respectively, when the mercury was made to have different temperatures. The next figure gives an idea of the sort of results obtained, the hot mercury being negative to both iron, copper, and to platinum. The values of mercury with the metal at the same temperature were obtained after several hours' cooling.

The results of the experiments made with mercury and lead are not given, as they are too indefinite to be at all trustworthy; and, indeed, in all the experiments with hot mercury there was a certain vagueness, since, although approximately the same values of the contact-potential differences might be obtained on successive trials at the same temperatures, occasionally a totally different value would be obtained for the same difference of temperature. It was at first thought that some effect might be due to a slow amalgamation of the induction plates arising from the vapour arising from the hot mercury; but this was not the case, as the electrometer deflection was the same immediately after the

rotation of the hot and cold mercury, or of the hot mercury and cold metal, as it was after they had been left for some time



DIFFERENCE OF TEMPERATURE.

subsequently to their rotation; that is to say, the large contact-potential difference was not in any way a time effect. The real expianation of the high potential difference and the abnormality of the results was that probably the main portion of the potential difference measured inductively was produced by an *invisible* film of oxide on the surface of the warm mercury, which possibly existed in spite of the fact that every effort was made to remove this oxide by distilling the mercury between every two sets of experiments.

It is to be observed, too, that when the mercury in contact with the copper plate has *cooled* down to the temperature of the

plate, the contact-potential difference is higher than the value we have previously obtained, viz., 0·308 volts, for the contact difference of these two substances. The discrepancy in this case may, however, have been produced by slight amalgamation of the copper strip in contact with the mercury having occurred during the hours of cooling.

Another anomalous result, which may also be regarded as a proof of the existence of an invisible film of oxide, is that the summation law does not hold true; that is to say, the electro-motive force of a combination is not equal to the algebraical sum of the various potential differences at the contacts, measured inductively, and which we have always found to be true in other cases when the experiments were conducted with as much care as was devoted to those made with hot mercury. For example—

Copper at 16° C., with *clean* mercury at 16° C., has a contact-difference of 0·308 volts.

Mercury at 16° C., with mercury at 26° C., has a contact-difference of 0·75 volts.

Mercury at 26° C., with copper at 16° C., has a contact-difference of −1·5 volts.

Therefore, if these numbers represented the true contact-potential differences, a thermopile consisting of two pieces of cold copper at 16° C., dipping into two vessels of mercury one of which was warmed up to 26° C., and which were joined together by a syphon tube containing mercury, ought to have an E.M.F. of half a volt. But, as was to be anticipated, we found experimentally that no such E.M.F. could be obtained with such a thermopile, unless there were an oxide present at the hot junction separating two of the metals and thereby introducing a high resistance.

Contrasting these results with those given in our papers read before the Royal Society (and which prove conclusively that when a cell is formed of a number of substances, solid or liquid, at the same temperature, its E.M.F. is equal to the algebraical sum of the various contact-potential differences *measured inductively*, in air, of each of the pairs of substances in contact in the cell), are *we not justified* in concluding that the values of the contact-

potential differences there given by us are the true values, and are not, as Dr. Lodge maintains, due to layers of oxide? At any rate we cannot admit that this view, which we have all along held, has been disproved by any evidence adduced by Dr. Lodge, and certainly not by any different values for these contact-differences being obtained by Mr. Brown in other gases than air—first, because even Dr. Lodge will admit that such experiments were vitiated by the layer of oxide formed; and, secondly, because we maintain that the summation law will not be found to hold with such results as Mr. Brown obtained, and which conclusively proves that such results do not represent the *true* contact-potential differences of the substances in the medium employed.

So far, then, I maintain, experiment is against Dr. Lodge's views. Next, I do not think it is quite right to say that the difference between Dr. Lodge and ourselves is a mere question of nomenclature, as has been urged by Dr. Hopkinson. Dr. Lodge considers that our view is not only unsupported by experiment, but is untenable from theoretical reasons. He further suggests the existence of an air effect as if it were a totally new suggestion, and one that had been overlooked by experimenters on contact-potential differences. Here I join issue with him, and maintain that he is quite wrong in imagining that the Volta theory as extended by Thomson has any theoretical absurdity in it such as he imagines to exist. The possibility of an air effect was fully foreseen by us. In our third paper, published in 1880, we drew especial attention to the possibility of such an air effect, and we showed, exactly as he has now shown, that the simple measurements of contact-potential difference by themselves gave no indication as to whether there is or is not an air effect. I do not think that any decisive experiments for or against the existence of a large potential difference existing between a metal and the air have yet been made, and beyond the advantage that accrues from eliciting people's opinions, I venture to think, with all due deference to Dr. Lodge, that his elaborate and most carefully worked out paper leaves the matter in exactly the same state in which it previously was.

Dr. Lodge speaks in his paper of M. Pellat's explanation of the

reason why a contact-potential difference between two metals is not necessarily accompanied by a production of heat when a current flows across the junction, as "a piece of unpleasantly plausible reasoning, which I myself have heard Professor Ayrton use, and which, when unexpectedly suggested, is so painfully benumbing that it is worth while to quote it, and to indicate its weak point." I can only imagine from this that Dr. Lodge has been so really benumbed by the argument that he has failed to grasp its meaning. Dr. Lodge's fundamental argument—the argument, in fact, on which his whole paper is based—is this: He *assumes* that in all cases where there is a considerable difference of potential at a junction, heat or cold must be developed when a current flows across that junction. He states what is quite true and well known—that practically no heat is developed when a current flows from zinc to copper across the junction; therefore he concludes that there can be no contact-potential difference at this junction like three-quarters of a volt. But this reasoning is based on a totally wrong conception of the Peltier effect, which I am astonished to find exists in the mind of a man with the scientific powers of Dr. Lodge. *The amount of heat generated when a unit current is sent through a junction for a second is undoubtedly the measure of the coefficient of the Peltier effect; but this coefficient is in no sense a measure of the contact-potential difference existing at the junction.* What the Peltier effect, at a junction of two substances at a given temperature, measures, is the product of the absolute temperature into the rate of variation, with temperature, of the contact-potential difference at that temperature; in other words, if Π is the Peltier effect at a junction which is at an absolute temperature t, and if V is the contact-potential difference at that temperature,

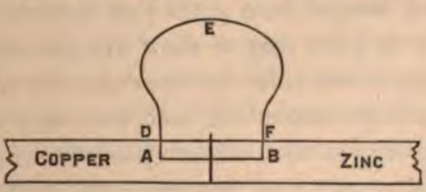
$$\Pi = t\frac{d\mathrm{V}}{dt}.$$

Hence the magnitude of Π does not measure V, but merely $t\frac{d\mathrm{V}}{dt}$, and the error of Dr. Lodge is of the same sort of order as saying that a train going at a very nearly uniform speed is necessarily *moving very* slowly because the change of speed per second is *very small.*

This disposes of the objection based on the smallness of the Peltier effect.

It may further, however, be objected that since the work done in moving a unit of electricity from a point of low to a point of high potential is a measure of the potential difference, it follows that if a zinc plate were really at anything like three-quarters of a volt higher potential than a copper plate in contact with it, a unit of electricity in passing from the zinc to the copper must give out as heat, or in some other form, the work which is a measure of this potential difference; and as no such a manifestation of heat is produced when electricity passes from zinc to copper, that therefore there cannot be such a potential difference as three-quarters of a volt between them.

But this reasoning, I wish specially to impress on you, is defective. It is quite right to say that the work done in moving a unit of electricity from a point of low potential *in the air* to a point of high potential, *also in the air*, is a measure of the potential difference; but this may cease to be true if the points be in different media. For example, it is quite conceivable that to move a unit of electricity from a point A in a copper wire to a point B in a zinc wire is practically nought, whether we simply pass straight across the junction along the direct path A B, or whether we first take our test unit out of the copper to a point D in the air close to the surface of the copper, then from the point D, along the path D E F, in the air to the point F close to the zinc, and, lastly, into the zinc from F to B.



To move the unit from D to F along the path D E F will require an amount of work which is a measure of the three-quarter-volt potential difference between D and F; but to move it from A to D out of copper into air, and from F to B out of air into zinc, may *exactly counterbalance* this amount of work, so that

the copper and zinc *may really* be at three-quarters of a volt potential difference as measured inductively by an electrometer, and yet there may be no work required to move electricity from a point in one metal to a point in the other, either across the junction or out of one metal through the air and into the other.

This is equivalent to saying that electricity may possess a form of energy different from *potential* energy, which latter is measured by an electrometer; so that, in order to completely specify the energy of a charged body, it would be necessary not merely to specify the quantity and the potential, but also the material of which the body was composed.

It is not therefore at all necessary to assume that there is any difference of potential between a metal and the air in contact with it.

Finally, therefore, I conclude—

1. Inductive experiments made on the contact-potential difference of zinc and copper show that the potentials of a point in the air close to the zinc and a point in the air close to the copper differ by about three-quarters of a volt.
2. The smallness of the Peltier effect adduced by Clerk Maxwell, Dr. Lodge, and others, to prove that the real contact-potential difference between metals must be small, and that therefore the greater part of the above three-quarters of a volt must be due to air effects, *does not prove this at all.*
3. *No* proof has yet been given that there is any air effect at all. It is quite easy to show how the potentials of zinc and copper may differ by three-quarters of a volt, and yet how no large amount of heat may be produced when an electric current flows across a junction.
4. No proof of any kind has been given that zinc and copper in contact do not really differ by three-quarters of a volt in potential.
5. The fact that the E.M.F. of a battery is equal to the algebraical sum of the actual contact-potential differences *of each* of the pairs of contact measured inductively at

one temperature, tends to prove that there is no oxide on the surfaces of the substances when their inductive measurements are made, and also tends to prove that the potential differences so measured are the actual potential differences of the substances themselves.

Mr. SWINBURNE: Of the two rival theories, the contact theory and the chemical theory, neither seems to explain the phenomena of electrolysis. If the electro-motive force depends on the contact of the metals, it should, as Dr. Hopkinson has already pointed out, be always the same, whatever be the chemical action. But it is not. As a good example, the case of a secondary battery may be taken: with sulphuric acid as electrolyte, it gives one electro-motive force; with caustic soda, another; and there are very many similar cases. The chemical theory, as commonly understood, is that the electrical work done by a cell is equal to the chemical work done in it. According to this, the electro-motive force should always be the same when the chemical work is the same. But it is not.

Dr. Lodge's theory is, of course, not the orthodox chemical theory. In calculating the Volta effect as due to affinity for oxygen, Dr. Lodge appears to have taken figures for the production of oxides which do not take into account the heat given out by the gas on changing to the solid state. J. Thomsen, who is generally Dr. Lodge's authority as to thermo-chemical numbers, does not seem to say much about the reduction to the solid state, but Berthelot lays great stress on these matters, and, though it might not affect Dr. Lodge's result, which is the difference between two supposed affinities, it is as well to remember that the zinc and copper ought, according to his theory, to be more than 1·8 and ·8 volts below the air. According to Dr. Lodge's theory the electro-motive force, in the case of the Volta effect, is due to the tendency of the oxygen and zinc to combine, and not to their actually doing so. If zinc has an affinity for gaseous oxygen, or for oxygen in the form of condensed films on the surface, why does it not combine with it? If this affinity is a force which can cause or sustain a proportionate difference of potential, where is the equal but opposite force? The case is not analagous to electrolytic polarisation, where the *radicles of the* electrolyte must be torn asunder,

for gaseous oxygen is certainly not an electrolyte. The case of two metals in oxygen is analogous, not to that of two metals in water or dilute acid, but in chlorine water. Again, if, instead of zinc, we take a metal that will combine with gaseous oxygen, what will happen? If the tendency to combine produces a difference of potential, we may surely conclude that actual combination either produces a current or raises the potential till the difference of potential counteracts the tendency to combine, and combination ceases. If a bit of sodium is insulated, and cut so as to expose clean surfaces, it does not appear to charge up to a point where its potential is equal to its affinity for oxygen and then stop, for the film formed corresponds to a large quantity of electricity. What becomes, then, of the charges of the oxygen particles? The sodium does not go on rising in potential till it discharges by sparks or brushes.

Would not a modification of Dr. Lodge's theory, in which water-vapour takes the place of oxygen, explain the Volta effect just as well? In the first place, water-vapour may be considered an electrolyte; so we get rid of some of the difficulties. Moreover, many metals seem to oxidise only in damp air. Dr. Lodge argues that oxygen is practically omnipresent, and is thus the cause of the Volta effect in as perfect vacuums as can be obtained, as well as sometimes in various gases. Water-vapour is, perhaps, even more difficult to get rid of, and its presence may be more freely assumed in the various cases mentioned than that of oxygen. The presence of ammonia in iron-rust is a curious point, and may some day throw some light on this subject. I do not advance any theory ascribing the Volta effect to the presence of water-vapour, or to anything else, but merely say that Dr. Lodge has not shown that it may not as well be due to water-vapour as to free oxygen.

Dr. Lodge goes on to give a table of what he calls "Volta effects in water," taken from Beetz and Clifton. If the Volta effect is due to the affinity of metals for gaseous oxygen, difference of potential due to an electrolyte can hardly be called a Volta effect, as the analogy does not hold. I do not quite see how *Dr. Lodge* calculates these effects. Does he assume that the

oxygen is free, or does he subtract its affinity for the hydrogen with which it is already combined? The change of state of the hydrogen must also be remembered. The numbers seem almost too wide of the mark to mean anything. The same remarks as to false analogy apply to Dr. Lodge's so-called Volta effects in sulphur and hydrogen sulphide; and the same remarks about the physical states may, of course, be made again.

Dr. Lodge has developed his theory of the Volta effect to account for the difference of potential in cells when electricity is driven through, and ascribes the difference of electro-motive force needed to do the same chemical work with different metals as electrodes to their different affinities for oxygen. If this theory be true, with the same metals and the same chemical work but different electrolytes, we should always need the same electro-motive force; but that does not seem to be the case. Why should the affinity for only oxygen be considered? Why should not the relative affinities of the metals, and of hydrogen, for the electro-negative radicles be taken into account? I think that, in the study of electrolysis, we must give up the old base and acid theory of the constitution of salts, and must eradicate all the notions derived from it. We must look upon zinc sulphate, not as a compound of zinc and oxygen and then of zinc oxide and sulphuric anhydride, but as a compound of the electro-positive radicle zinc and the electro-negative radicle sulphion. Of course this classification becomes somewhat complicated in many cases, for radicles may be themselves split into pairs. Take, for instance, the case of potassium permanganate, or the case of the solution of zinc in caustic soda, where the zinc seems to become the electro-positive part of the electro-negative radicle. Some time ago Dr. Lodge published some papers on secondary batteries, according to which oxide of lead was formed on discharging the cells, and the affinity of lead and sulphion was not taken into account, the formation of sulphate of lead being regarded as a sort of secondary action. I thought every one who has worked much with secondary batteries, including Dr. Lodge, had given up these ideas long ago, but he seems still to think chiefly of the affinities for oxygen. Another idea, which *obscures the subject* very much, is the nascent gas

theory; all electrolytic action is said to be due to nascent oxygen and nascent hydrogen. It does not at all follow that since hydrogen is evolved when zinc is put in sulphuric acid in the presence of, say, an iron persalt, that the reduction of the iron persalt is caused by the nascent hydrogen. Reduction can take place without hydrogen being evolved, and when there is not enough electromotive force available to evolve hydrogen at all. We can only say that the chemical action taking least energy takes place; that is to say, in the case of the iron persalt, that the moving electricity passes by reducing the persalt when it is available, and when it is not by evolving hydrogen. Even the terms "oxidise" and "reduce" will depart further than ever from their original meanings as the electrical theory of chemistry develops. I have tried a few experiments with small currents passed through cells with different electrodes and different electrolytes, so as always to do the same chemical work—that is, to evolve oxygen and hydrogen—but have not made enough to publish the results yet, and I do not advocate any theory now.

This last method seems to be one of the simplest and most promising. It would also be interesting to put the cells in a calorimeter and note the effects on passing a given quantity of electricity through at different rates. Some of the unexplained discrepancies may be due, as Dr. Hopkinson points out, to the electrolytes being heterogeneous, and it would have to be found out whether the heterogeneity exists only when there is a current, as in the case mentioned, or even when electrodes are kept at different potentials in the liquid without a current. The heterogeneity is dwelt upon in "Miller's Chemistry," if I may trust my memory.

It would be interesting to know how the advocates of the contact theory explain the Peltier effect. If a contact-force of a volt or two does not cause heating or cooling on passing electricity across a metallic junction, a few microvolts cannot do so either.

If the contact-force which causes the Peltier effect be due to the chemical affinities of the metals, the contact-force between sodium or potassium must be enormous, as Dr. Lodge remarks; *but we know* these forces balance in a circuit of metals at the

same temperature; so in a circuit of iron, mercury, iron, sodium, we ought to have an enormous Peltier effect between the iron and mercury, or iron and sodium.

The importance of the discovery of the laws of electrolysis cannot be overestimated, as it would give a means of determining many thermo-chemical values which now give great trouble. In fact, it is probable that electricians will do more to advance the science of chemistry in the next few years than chemists themselves.

Professor S. P. THOMPSON: We have heard more than one speaker to-night announce himself as remaining still an adherent of the contact theory. I should like just to ask what, after all, the contact theory is, and what evidence there is for it. If I understand the matter rightly, the contact theory is this—that the seat of the electro-motive force in the circuit is not at the place of chemical activity where the energy enters the circuit, but at the place where there are two mutually inert metals in contact one with the other; and the evidence for this—the only kind of evidence, so far as I am aware, that can be produced in favour of this most improbable doctrine—is that when you take a piece of metal surrounded by air—an oxidising medium, be it observed—and bring it into contact with a piece of a different metal also surrounded by the air, there is an observable difference of electric potential between two points in the air respectively near to these two metals. Does that—can that—prove that there is a difference of potential between the two metals? Does it not equally prove that they, being respectively at different potentials from the air, are themselves at one common potential? There were some experiments referred to to-night (and I regretted to observe that several speakers who referred to them were disposed to speak slightingly of their importance), made by Mr. Brown, of Belfast, that seem to me to be of the very highest significance, though they are avowedly only of a qualitative character. Mr. Brown made experiments on the differences of potential to be observed at points near metals in contact, when the surrounding medium was other than air, and he found the differences of potential not to depend so *much on the* metals that were in contact with one

another as on the medium which surrounded the metals. Now, the exception that has been taken by the upholders of the so-called contact theory to Mr. Brown's experiments is that these media—chlorine, sulphuretted hydrogen, and the like—act chemically upon the metals that are placed in them. This is perfectly true, but so does air. If one set of experiments are inadmissible because of chemical action, so are also the other set. I therefore fail to see why experiments made on metals in air are a whit more conclusive than those made on metals in other gases. Yet that is precisely what Professor Forbes assumes in still upholding the so-called contact theory. I believe the theory to be fallacious, and founded upon experiments which are fallacious, simply because they are made in an oxidising medium. Until experiments are made on metals in the entire absence of any chemically-active medium, I shall decline to believe that the so-called contact phenomena — which Mr. Brown has shown to depend on the medium, and to vary with the medium, are not due to the medium. I strongly support Professor Lodge's views, and believe the position he has taken is unassailable on the facts.

Professor AYRTON: Professor Thompson is mistaken in thinking that the contact theory makes the seat of the electro-motive force at the junction of the metals; on the contrary, what it says is that the electricity rises in potential at this junction. The great objection to Mr. Brown's results, and, indeed, in my mind the proof that they were vitiated by films of oxide, is that the sum of the contact-potential differences as measured by him inductively in, say, chlorine gas, would not be equal to the electro-motive force of a cell built up of the pairs of materials he used.

Professor S. P. THOMPSON: I am quite ready to admit that Mr. Brown's experiments are imperfect; in fact, they do not go far enough. For my own part, I do not doubt for a moment that, if repeated with great care, they will show exact agreement amongst themselves. Take the case of copper and zinc. Zinc appears to be positive with respect to copper in air—an oxidising *medium*—and *it* is positive to copper in water or dilute sulphuric

acid, both of which are oxidising media; and if the separate differences of potential between a series of metals in air are taken, the first of them being zinc and the last of them copper, the sum of the differences will be found equal to the whole difference of potential. Take the same series of metals, however, and surround them fairly with any other medium than air—say a sulphurising medium—and measure their separate differences of potential in that medium: I have not the smallest doubt that the potential-difference from first to last will be equal to the sum of the separate differences in that medium. To think otherwise would be to doubt the uniformity of the great thermo-dynamic law * which includes within its operations the distribution of electric potentials in a circuit.

Professor AYRTON (*note added in reply, July,* 1885): That the electro-motive force of any combination must of course be equal to the algebraical sum of the potential-differences actually existing at the time at the various contacts, is a self-evident fact; but that is a totally different thing from saying that the electro-motive force of a combination is equal to the algebraical sum of the contact-potential differences, each measured *inductively* with a Kohlrausch arrangement. This latter is not self-evident. We find that it is true when the experiments are carefully made in air; but it will not, I believe, be found to be true when the experiments are performed as Mr. Brown conducted them, because films of oxide, sulphide, &c., are present on the surfaces of the substances when the induction experiments are being made, but are not present between the substances when they are joined up together to make a cell. In fact, as stated in my remarks on the results of our experiments on the contact-potential difference at various temperatures, the fact that the electro-motive force of a

---

* *Note added, June,* 1885.—If the sum of the differences of potentials of a series were not equal to the whole difference of potential from first to last, then it is clear that a self-acting electric perpetual motion would result by uniting the first and last members of the series, the greater difference of potential maintaining a current against the lesser—which is absurd. This proof may be considered the counterpart, in the thermo-dynamic theory of the voltaic cell, of the well-known theorem of the perfectly reversible engine in the thermo-dynamic theory of heat.—S. P. T.

cell is equal to the algebraical sum of the contact-potential differences of the various pairs of substances at the same temperature, measured inductively in air, is to my mind a strong proof that these measurements do really give us the true contact-potential differences, and that they are not vitiated by films of oxide, sulphide, &c.; and, on the other hand, I think that when we find that this is not the case, as we found when experimenting with hot and cold mercury, we are justified in concluding that disturbing oxides, &c., are present.

Professor Oliver Lodge: With regard to the point last raised, I go further than Professor Silvanus Thompson. In his answer to Professor Ayrton, he says that, in order to get the total E.M.F. of a current equal to the sum of the Volta forces for all the junctions of that circuit, you must have your circuit in the same medium as that in which you measured your Volta forces. I go further, and say that no matter what medium surrounds the substances at the time their Volta forces are being measured, provided it is *one* medium and not half a dozen different ones, the sum of the Volta forces must equal the E.M.F. of a circuit of those substances, whether that circuit be in the same medium or not. It is a mere matter of elementary algebra.

If the metals are not clean, I regard the dirt on their surfaces as medium different from air. In this case the summation law will not hold, because there is no one medium in which all the Volta effects are being measured, but a different one for each metal. The summation of the Volta forces coming out equal to the E.M.F. of a circuit proves, therefore, that the metals used were reasonably clean, and not scummed over with oxide or dirt; and this is, probably, the point that Professor Ayrton intended to bring out. In this, therefore, I perfectly agree with him.

I am obliged to Mr. Swinburne for his statement with regard to certain thermo-chemical details. I do not bring forward these data as certainly accurate—they are the best yet known, and my authority for most of them is Naumann's edition of "Gmelin-Kraut;" for some of them "Watts' Dictionary." Perhaps I can appreciate Mr. Swinburne's remarks better when I see them *in print*; but so far as I understand him at present, he

misapprehends my main position, viz., where I regard the Volta force as due to a chemical *tendency*, not to chemical *action*.

Return now to the beginning. Professor Perry's paper first of all charges me with a tendency or determination to adhere to the view that I have taken up in spite of all experimental evidence whatever. I suppose this is only to give spirit and verve to his opening sentences. I venture to assume that he does not really think this of me, else we should not be so friendly as we are. I will, however, just say that I do not assert that the Volta effect is produced by the air, in opposition to all evidence; for I say that the evidence on the other side is not forthcoming. No doubt it is a thing that can be settled by experiment ultimately, but at present a crucial experiment, which will satisfy everybody, seems to me almost hopeless. Professor Ayrton says I have adduced no experiments! I have adduced all I could lay my hands on throughout the century. They are not mine; some of them are his! There is no lack of experiments on the subject, and it seemed to me better to try and digest them, and to draw out their moral rather than to bring forward a number of fresh ones. There may be some very admirable and final experiment possible, but I do not feel myself competent to make it. I suppose Professor Perry intends a joke when he talks about Professor Exner as my "chief," because I do not even agree with Professor Exner. He evidently feels that he has rather a bad case, and may strengthen it by leading you away from the point if possible.

I must go into some detail with regard to Professor Perry's ideas of "potential," because here is where the main gist of our difference lies. His notions cause him to feel a difficulty not only about electrical problems, but about water problems also, and even about such apparently simple matters as the transmission of energy by means of a rope or a shaft. But when one gets into difficulties about such elementary things as these, is it not time to begin to think that one's fundamental notions are perhaps confused? Now, I venture to think that his fundamental notions are confused somewhat; and, to bring this out, I need not discuss electrical matters, for simple hydrostatics will serve. It is

a bold thing for me to differ from Professor Perry in hydraulics, when, as every one knows, he is an authority on the subject, while, as everybody perhaps does not know, I am not! I do not, however, pretend to set my knowledge of hydraulic machinery against Professor Perry's. The points where we differ are points involving no knowledge at all but merely clearness of idea, and at the bottom are questions of definition.

Professor Perry draws this diagram, of an irregularly bent closed pipe, upon the board, round which water can be pumped continuously in a circuit. He says there is one potential at A and another at B higher up, and that water rises in potential in going from A to B, although there is no force acting in the pipe A B to help it up, and hence that a difference of potential may exist without any force or E.M.F. I say that neither electricity nor water can flow up hill in this way, unless it is in that place pumped. If there is no pump between A and B, and if water is flowing from A to B, then I say that, *ipso facto*, the water at B is at a *lower* potential than that at A; and that if B was really at a higher potential it would be impossible to get the water to flow from A to B, except by a pump in A B. That is exactly my point, that the existence of a difference of potential *proves* the existence of an E.M.F.

How does Professor Perry prove that B is at a higher potential than A? Not by measuring the work done by the water in flowing in the pipe—for he knows well enough that no work is thus done (barring friction)—but by carrying up some other distinct water not belonging to the vessel, from A to B, outside the pipe.

Well, then, this evidently shows that we do not mean the same thing by "potential," and that, as Dr. Hopkinson has just now said and Professor Fitzgerald last time hinted, there is a difference of definition between us which prevents our seeing the same thing with the same eyes and of appreciating each other's point of view. Well, I fancy I know what he means; and probably he knows what I mean, only he does not agree with it. Take this tumbler of water. Professors Ayrton and Perry would say that the potential of the water at the bottom of the tumbler

is greater than that of the water at the top—either greater or less, I am not sure which——

Professor PERRY (*interposing*): We say *level*, not "potential."

Professor LODGE (*resuming*): You did say "potential," and it is wholly with reference to potential that this hydraulic analogy is of any assistance or value. If you only say *level*, no one can doubt that a point below another is at a lower level. You have throughout used the word "potential," in the electrical case at any rate, and without it no water-analogy is any assistance. Moreover, it is legitimate enough to define "potential" in such a way as to make the potential at the bottom of a vessel less than at the top—gravitation force maintaining the water still in spite of it. But it seems to me much preferable to define it so that the potential at top and bottom *would* be different except for the weight of the water, which equalises it. Professor Fitzgerald, however, in his remarks a month ago, supported the former definition.

I say that the potential of the water at the bottom of the vessel must be at the same potential as that at the top, because, if it were not, the water could not keep still. I define *potential* as the potential-energy of a pound (or cubic inch) of water at that point. I mean, therefore, by "potential" what Professor Perry would call "head"—height of *free surface* above the sea, not height of the water at the point considered only.

Professor Perry would say that the bottom of the water is at a less potential but a greater pressure. He also would define *potential* as the potential-energy of a pound of water; but then he measures his potential-energy in a peculiar way, viz., by the work done in bringing the pound from the sea to a point on the same level just outside the vessel. The extra work required to put it *into* the vessel, which I should equally call part of the potential-energy of the water inside, he calls *pressure-energy.*

And this is just where what I call his confusion comes in. He has expressed himself forcibly with reference to me; I do the same with reference to him—it saves time. He knows well enough that pressure is not a form of energy, and yet he splits up the *potential-energy of* water into two parts—"potential-

energy" and "pressure-energy;" and he does just the same in electricity. I only know two fundamental kinds of energy—potential and kinetic—and all that is not kinetic, or, let us say, not obviously kinetic, I call potential. This is the common custom, of course, and there must be some reason which makes Professor Perry go out of his way to insert a third kind, called "pressure-energy." What is that cause? Can it be this?—In one place he considers a rigid vessel choke-full of an incompressible liquid under pressure; and he may use this to show a discrepancy in my definition, thus: "Such liquid has no more energy than if it were under no pressure [*granted*]; but, according to you, it is at a high potential, because its 'head,' as measured by a pressure gauge, is high [*granted also*]; and yet you define "potential" as the potential-energy of unit quantity." But these statements are not irreconcilable, because, remember that if the removal of the measuring-water alters the condition, then you are not measuring the original condition, but some other. The water introduced or removed, to measure the potential, must be infinitesimal in quantity. A drop of water let out of the above vessel relaxes the pressure and lowers the head: the potential of the remainder is very small; and the potential-energy of the whole must of course be reckoned by integrating the product of quantity of water and potential at each instant.

The vessel may, in fact, be considered as having an infinitely narrow pipe rising to a great height, and full to the brim, but whose surface-level instantly falls if any water is taken out of the vessel.

The thing is therefore perfectly consistent and simple, when regarded from my point of view.

Professor Perry's energy formula $\frac{v^2}{2g} + 2{\cdot}3\,p + h$ well exhibits the confusion. Surely in the form $\frac{1}{2}\,v^2 + g\,\mathrm{H}$ it is much more simple; where H means surface-level, as indicated by a gauge inserted at the point considered. Moreover, there is an odd mixture of units in the formula: the height is in feet and the 2·3 in pounds-weight per square inch; but that is a detail.

I have not the least intention of denying that the formula is

a useful one in dealing with the flow of water through pipes. I have no doubt that it is. All I object to is the calling of its first term, "kinetic-energy;" of its second term, "pressure-energy;" and of its third term, "potential-energy;" when really the second and third terms together constitute the potential-energy. The three terms are evidently convenient if dial or mercury pressure-gauges are used, or imagined to be used; if water-gauges were used, the two-term formula would be as convenient, reading all the surface-levels from one datum line.

Now, returning to the diagram, and considering the water at rest, I say that there is no difference of potential at all between the water at A and at B, *unless the channel is an open one, so that the water has a free surface.* In a closed pipe the potential of water is quite independent of ups and downs in the pipe, you may twist it about how you please; the water flows quite easily, and does no work in going either up or down. There, are in fact, no real hills in a closed pipe, only apparent ones to an outside observer.

If the diagram represents *an open channel,* then water at B is certainly at a higher potential than water at A, but now how are you going to get water to flow from A to B? You can't, unless you put a pump there. A pump somewhere else is no use. That is what I say. *Where your difference of potential is, there your pump must be.*

Professor Perry, with his closed pipe, may indeed put his pump anywhere; or, speaking electrically, the seat of E.M.F. has no connection with a contact-difference of potentials. But I say that his "difference of potential," and Fitzgerald's "difference of potentials," is no difference, and that wherever there really is a step of potential *there* must the E.M.F. be.

Just one observation with regard to the meaning of E.M.F. Professor Perry asserts that Maxwell's definition of it renders it self-evident that its seat is where energy is taken into the circuit, and he admits that at a zinc-copper junction there is no E.M.F. in this sense; "but," he says "there is a contact-force, the cause of the contact-difference of potential."

I *don't know what he* means by a contact-force acting on

electricity which yet is not an electro-motive force, but whatever he means I deny its existence; and I deny the existence of any difference of potential in a conductor, except where there is an electro-motive force.

It may be a question of definition, as Dr. Hopkinson says, but these questions of definitions are not unimportant; they often lead to differences about facts, as in this case they do; and the settlement of them may make all the difference between clearness and fogginess.

Now, Professor Perry and Dr. Hopkinson have, I think, either asserted or allowed the assertion, that though no work is done in going from a point in zinc to a point in copper *through the junction*, yet there is work done in travelling between the same points *through the air;* and that it makes all the difference which way you go. So I understand one of the statements made.* Well, this will not hold for an instant. The whole idea of a potential is that it shall depend on the state of affairs at each point considered, and shall not depend upon the path of arriving at it. According to my notion, every point of a piece of clean isolated zinc is about 1·8 volts below the potential of the air at its surface. A piece of clean isolated copper is similarly ·8 volts below the air. Put them into contact—the potential of the zinc rises and that of the copper falls, until they are both at the same potential, viz., about 1·3 volts below the undisturbed or distant air, if the two metals are of equal size. But the potential of the air is not undisturbed by this process: that near the zinc is still 1·8 volts above the zinc, and that near the copper still ·8 volts above the copper; hence there is a slope of potential equal to about 1 volt in the air itself, viz., from the air near the zinc to that near the copper. This slope of potential is, in fact, exactly what electroscopic experimenters have observed and measured throughout the century and called "the contact-difference of potential between zinc and copper."

Now travel from a point in zinc to a point in copper, carrying

* In the proof I see that Dr. Hopkinson's statement was different from this, and was perfectly correct. I only leave this paragraph in, therefore, for the sake *of explication*—not controversy.

electricity with you. Go first wholly through the metals themselves, across their junction—no work is done. Go now through the air; on crossing the zinc-air boundary you have to climb a sudden precipice, doing 1·8 units of work; the rest of your journey is down hill; 1 unit of work is done on you while traversing the gentle slope through the air, and ·8 more when stepping down the final precipice into the copper. Hence no resultant work is done in this case any more than in the former. Or take it backwards. In going from copper to zinc, direct through the junction, there is no work done: in going from copper to air there is work done, in going through the air there is work done, in going into the zinc there is negative work done; but these opposite works neutralise one another, and the whole work going through the air is 0—just the same as when going through the junction.

All that I have said about the observed Volta effects being really air effects has been said much better by Maxwell. I do not claim any statement that is to be found in Maxwell, and I have got a great many of my ideas from him. Whatever he has said is there in his book to be seen. Sometimes the idea occurs to one first, and then one finds it there afterwards. Sometimes I find Maxwell half-unintelligible till one has thought over the matter a long while for oneself, and then I find my ultimate ideas quite clearly expressed all the time.

Dr. Fleming has asked for some crucial experiment to be suggested. The experiment which naturally occurs to everybody is to repeat electroscopic experiments in some neutral medium; but unless you could be sure of the absolute purity of the medium and absence of oxygen molecules, the result would not be satisfactory. Absolutely pure gas is extremely difficult to get, and Professor Fitzgerald accordingly suggests some liquid such as benzine or paraffin. But even then I believe you would have a poor chance of satisfying everybody. I do not see how it is to be done. If the Volta effect came out *nil* in such a medium, Professor Ayrton would say it was because the plates had got a nasty scum on them.

There is another kind of crucial experiment which may be

suggested, viz., the determination of the alloying-heat for metals. According to my views the heat of formation of brass is very small, and is calculable from the size of atoms and the Peltier force at a zinc-copper junction. According to Sir W. Thomson the heat of formation is considerable, and is calculable from the size of atoms and the zinc-copper Volta effect.

By the way, in speaking of Sir W. Thomson's views throughout the paper, I have used his name much as politicians use Mr. Gladstone's; he is too big a man for one to need be particularly careful in referring to him, and obviously he is not in any way committed by my fancy that certain views are his. No one can say what any man's views are except the man himself. I can only form a rough estimate of Sir W. Thomson's views from his published papers, but his magnitude makes it incumbent on one to try to estimate those views on every subject, and to most carefully reconsider one's own with reference to them. This I have not neglected to do.

My chief argument, and main proof that my views are correct, lies in the fact that I can calculate the Volta effect for any ordinary pair of metals, in absolute measure, from their heats of oxidation, and that the results of calculation agree very fairly with the results of experiment.

Professor Forbes: No.

Professor Lodge: Well, it is of course a matter of opinion how far the agreement is satisfactory. It is not complete and exact, I admit; but the numbers are all in the paper for reference and comparison, and I must say that they seem to me very fairly close and satisfactory, considering the extremely different conditions of an electroscopic and a thermo-chemical experiment.

Professor Forbes has spoken of the question as a metaphysical one. I don't know where he gets his "meta" from. The subject, I should say, is decidedly physical, if anything is. One must get one's definitions right and then the whole matter will be simple, and I am sure that Professor Forbes will find his ideas much simplified if he adopts what I consider the ordinary definition of "difference of potential."

Professor Ayrton's remarks with reference to mercury seem to have no special bearing on the subject. The Volta effect between

ot and cold metals is due to coats of oxide, as Knott made out. But he goes on to say that I have not only made a question of definition of it but I have made a question of right and wrong of it, and said that the seat of E.M.F. is not at the zinc-copper junction. Yes I have, and I stick to it; and if Professor Perry gives way on that point, and says that though there may be no E.M.F. yet there is a contact-force there, then I will go further and say there is no contact-force, and that difference of potential between zinc and copper is absurd. If you mean by "potential" what I mean, it *is* absurd, unless there is a force at the junction. If there were a momentary difference of potential between zinc and copper in contact, electricity would at once run from high to low. It would be absurd to say there was a permanent electrostatic difference of potential in a conducting mass. If you mean by "the potential of a point" the workdone in bringing up unit electricity to that point, whether that point be inside a conductor or not, then you cannot have a difference of potential in a conductor, except where there is contact-force or E.M.F. to maintain it.

I have omitted any mention of the Peltier or Seebeck forces, and perhaps it is hardly worth while going into that to-night. I have, for simplicity, throughout considered them zero, in agreement with Professor Perry's initial suggestion. Perhaps, however, I may be allowed to draw a diagram, to show how I regard the facts of a thermo-electric circuit, in answer to Professor Forbes.

Take a copper-iron circuit, with the hot junction at some temperature below 275° C. Then there is a small electro-motive force at the hot *junction, as* shown by the arrow, and a larger

opposing force at the cold junction. There is a small force the copper from cold to hot, and a larger force in the iron from hot to cold. If the temperature were made uniform this last pair of forces would vanish; but the first pair would not vanish, they would only become equal. Inequality of temperature excites the one pair of forces, and renders unequal the other pair; and as the resultant of the system of forces we have a thermo-electric current as first observed by Seebeck; though in special cases the resultant may be zero, as noticed by Cumming.

All these forces may therefore be called "Seebeck forces," or "thermo-electric forces," or "true contact-forces."

If, now, a current be made or allowed to flow round the circuit heat is destroyed or generated at the various junctions—a fact discovered by Peltier at the copper-iron junction, and by Thomson inside each metal itself; and these generations of heat or cold are called the "Peltier" and "Thomson" effects, respectively.

I therefore often distinguish the one pair of Seebeck forces by calling them "Peltier forces," and the other by calling them "Thomson forces." If the temperature of the circuit be uniform the Thomson forces are *nil*, but the Peltier forces still exist, though balanced, and may still exhibit the Peltier effect whenever a current passes. A Peltier force only vanishes at absolute zero and at the temperature known as the neutral point, which, for copper-iron, is about 275°, and above that point changes sign.

A Thomson force thus depends altogether on difference of temperature; a Peltier force depends neither on difference of temperature nor on the existence of a current, it depends simply on absolute temperature. The Thomson *effect* and the Peltier *effect* both, of course, depend on the passage of a current. This will explain the senses in which I use the terms.

In a certain sense the Peltier force may be said to exist all round the current, for Sir William Thomson has shown that the E.M.F. of a circuit is $\int \frac{J\,\Pi\,dt}{t}$, but the Π used in this sense I prefer to call the Peltier *function*.*

I wish to call attention to my suggestion for nomenclature.

* See, for further illustration, the *Philosophical Magazine* for June, this year.

I propose to limit the symbol Zn/Cu, or Sb/Bi, or A/B, to the true contact or Peltier force at a junction of two metals; and to use the symbol Zn/air/Cu, or Zn/water/*Pt*, or generally A/medium/B, for the Volta effect between A and B in the specified medium. The total effect, observed electroscopically, viz., medium/A/B/medium, will then obviously be the sum of the two, which it undeniably is.

No one, I believe, doubts the applicability of these expressions, or at least of this last expression; all the question is, whether A/B is the most important part, as Professor Perry holds, or whether medium/A—medium/B is the only important part, as I hold.

Before sitting down I wish to look at the matter from the point of view of Professor Poynting's very admirable recent paper. It had not been customary to pay attention to electrostatic-potential in current electricity, but Mr. Poynting has shown that these equipotential curves are the paths of the energy, and as such are of great interest.

Now, it is not the most obvious thing in the world how to draw these curves for a voltaic cell, and it has not yet been done. But if I proceed to draw them from the point of view of my theory—viz., if I postulate a considerable difference of potential between zinc and air, and a less difference of potential between copper and air, and then draw the curves of electrostatic-potential for a closed circuit containing a voltaic cell—the curves will indicate clearly and exactly why there has been all this controversy about the seat of the E.M.F., and they will, I hope, reduce the misunderstanding and difference of opinion to zero.

For we find that while all the lines start from the common surface of zinc and acid, yet many of them invest the metals so closely as not to be perceived in the surrounding space or air. All we then perceive are those which spread out, viz., those which invest the zinc but not the copper.

If we suppose a difference of potential between zinc and air of 1·8 volts, and between copper and air ·8 volts, and if we draw a line for every tenth of a volt, we shall have a total number of 18 lines, all investing the zinc closely, like the coats of an onion.

Professor Perry originally pointed out to me that, according to my notions, these lines would form a coat to the metal. It was his arguments, based on these lines, that set me to try to draw them. It was a distinct advantage to me to talk the matter over with him. Of these, 8 invest the copper similarly, but the remaining 10 cannot, and accordingly they spread out into the air and are recognised by electroscopic experiment.

But whence do they begin to spread out? From the junction of zinc and copper! The whole 18 have coated the metal up to this point, but beyond it only 8 can coat it; hence it stands to reason that 10 must be given off from this point.

Electroscopic experimenters, tracing these lines home, have found them all converging on the zinc-copper junction, and have jumped to the conclusion that they started thence. But they did not start thence; they started at the zinc-liquid junction, and only *began to spread out* at the metallic junction.

Here is the diagram, with every alternate tenth-volt line drawn. The liquid of the cell is not indicated, because it is simplest to assume the liquid to behave much like air, as water and dilute acid are known to do.

Note, that the copper is everywhere separated from the air by 4 lines, *i.e.*, by ·8 volt, and the zinc by 9 lines, according to hypothesis. Note, also, 5 lines branch out from the junction of zinc and copper (though one of them, the 1·8 volt line, only separates the zinc from the liquid of the cell), and that these correspond to a Volta effect at the junction of about 1 volt.

I assume that with absolutely clean zinc the Volta effect Zn/air/Cu, would not fall far short of its theoretical value, 1 volt. It has been measured as high as ·92.

It is hardly necessary to say that the distribution of potential through internal or external resistance is an arbitrary matter, depending on the ratios of their resistances. I happen to have chosen a circuit whose internal resistance is two-thirds the external.*

The remarks of Mr. Heaviside, which he had not time to give

* For further information respecting this diagram, see *Philosophical Magazine*, June, 1885.

at the meeting, take the form of a supplementary paper agreeing in many respects with my own. Mr. Heaviside, being merely concerned with setting forth his own views clearly, instead of adducing arguments and combating other people's, has probably succeeded in making the matter more easily intelligible than I have done. Hence a perusal of this part of his communication may be a useful preliminary to the reading of mine; though, naturally, I do not agree with quite all he says.

A question sent me after the meeting, by Mr. Hugh E. Harrison, may, with my reply, also help to elucidate the views advocated :—

"The Hammond Company Electrical Engineering College,
"2, Red Lion Square, London, W.C.,
"*April* 28, 1885.

"DEAR DR. LODGE,—I had one or two questions to ask you the other night, at the Telegraph Engineers, but I rose at the same time as Mr. Swinburne, and after that the discussion was practically closed to outsiders. I made an attempt in the coffee-room to speak to you, but your bodyguard there resisted all my feeble efforts.

"My excuse for writing is, that I have been in the habit of giving to my classes what I have gathered of your ideas from Professor Foster; and I should now like to introduce your explanation of the Volta effect if I were quite happy about it.

"The chief point I don't see is briefly this:—Suppose you had a piece of perfectly clean zinc in vacuo at the potential of the earth, but insulated; if air were now let into the receiver I take it that, according to your idea, the zinc would chemically attract atoms of dissociated oxygen, and thus, while not altering its own charge in any way, would surround itself with a film of negatively-charged gas and so lower its potential to a point such that the chemical attraction for dissociated oxygen is exactly balanced by the negatively-charged film. So far the whole thing is clear enough, and such a piece of zinc would show in the surrounding air a negative potential of 1·8 or thereabouts, by the ordinary electrometer method.

"But, as a matter of fact, a piece of clean zinc in air is apparently at the same potential as the surrounding atmosphere, which means that it has got a positive charge equal in quantity to the negative charge on the oxygen film round it; so that the zinc itself has a potential very slightly above zero, since there is of course some distance between its surface and the gaseous film.

"There now being no electrical reason why a stray oxygen atom should not stick itself unto the mass, why does not chemical attraction again come into play, until the potential of the neighbourhood again falls, owing to an accession of more oxygen?

"The above will, I hope, sufficiently indicate the tangle of ideas it is intended to show.

"To sum up in a few words, so long as a piece of zinc remains unoxidised I do not see how a state of equilibrium can exist between electrical and chemical forces, unless the air in the neighbourhood is at a negative potential.

"The other difficulties merely concern the hydrostatic analogies, so are not of much real importance.

"Hoping you will forgive the length of this letter,

"Believe me, yours very truly,

"HUGH ERAT HARRISON."

"University College, Liverpool,
"1st *May*, 1885.

"DEAR MR. HARRISON,—I have great pleasure in answering your well put difficulties as far as I can.

"I do not picture the action of the piece of zinc in the precise way you describe. Start with Zn in vacuo and let in air, the Zn and the O pull at each other, and this pull, *ipso facto*, causes Zn (or the E in the Zn) to be at a lower potential than it was before.

"Nothing to do with charge, nothing to do with a layer of neg. oxygen. My point is, that if the Ox atoms were able to move any nearer the Zn than their normal distance *then* they would constitute a *positive* charge (*i.e.*, a thinner negative layer), but that so long as they surround the Zn equally all round they can't move up at all or charge it either way without compressing *electricity*, so to speak: which is impossible.

"When, however, by contact with Cu, a path is opened for the neg. E to escape, then they do move up. The zinc surface instantly becomes + charged, and the potential is thus raised. The potential is now the resultant of two things—the pull, as before (or even stronger because nearer), and the surface charge.

"The hypothetical neg. pot. of the zinc, produced by the straining air, is not a thing that can be detected by electrometer methods, for they only give the pot. of the air.

"I say there *is* an electrical reason why O can't attack pure zinc not in contact with any other conductor—viz., the equal pull all round, and the incompressibility of E.

"The zinc is quite uncharged until contact is made with some other body, when it gets a charge, which has been investigated by the electroscopic people as if it were a change of potential produced by the contact. So it is; but not in the way that they think.

"I don't suppose any equilibration between chemical and electrical forces *before* metallic contact—only after.

"I hope that, if you can read this, it may explain my point of view. Whether or not you feel disposed to agree therewith, is, I know, another matter.—Yours very faithfully,

"OLIVER J. LODGE.

"Hugh Erat Harrison, Esq."

SOME REMARKS ON THE VOLTA FORCE AND SEAT OF ELECTRO-MOTIVE FORCES QUESTIONS, AND ON IMPRESSED FORCE AND POTENTIAL IN CONDENSER CIRCUITS.

By OLIVER HEAVISIDE, Associate.

Professor Lodge has done me the honour of mentioning me, in the course of his learned memoir on the "Seat of the Electromotive Forces in the Voltaic Circuit," as having published some similar statements to some of his. It may be of interest to the members of this Society to learn what they were. The views to which Professor Lodge's researches have conducted him are so very *similar to mine, except* on some purely speculative points,

that it might be merely necessary to point out these points. But as my previous remarks on the subject do not extend to any great length, I may as well quote the article containing them. This will be advantageous, because, so far as the Volta force is concerned, they are disconnected from the historical matter which forms so interesting a part of Professor Lodge's memoir, by publishing which, together with the valuable data he has been at so much trouble to collect, he has conferred a great benefit on the present generation of electricians. Being thus isolated, as well as from the subject of thermo-electricity, they may enable some readers who have not given much attention to the matter to more easily understand the plain course of the argument apart from the speculative points which are incapable of present verification. I shall add some additional remarks on the general subject, and wind up with a statement of what I consider to be the correct mathematical representation of the relations of impressed force and potential in condenser circuits, when there are impressed forces in the dielectrics as well as in the conductors—relations which are perhaps not yet fully recognised, if I may judge from some remarks of Professor Lodge's on the total unlikeness between the Volta-force conditions and an ordinary condenser investigation. But there is nothing revolutionary in the case; and when we make use of Maxwell's most admirable theory of dielectric currents and displacement, the case of impressed force in the dielectric becomes as easy to follow as when it is in the conducting part of a circuit. The theory of current in the dielectric I consider to be certainly true, however difficult its experimental verification may be (though far more difficult to disprove), and to be the natural outcome of Faraday's way of looking at things electrical.

I may premise, in the first place, that, like many others, I had been for many years profoundly dissatisfied with the paradoxical state into which electrical theory, in other respects so consistent all round, was thrown by Sir W. Thomson's conclusion, from his experiments with his wonderful electrometers, that old Volta was right, and that there could be no doubt the whole thing was simply *chemical action* at a distance. He proved decisively that the

setting of two metals in contact did charge them like a condenser, and concluded that the electro-motive force was situated at the junction of the metals, founding upon this a notable calculation of the probable size of atoms by the manufacture of brass. Now, if it had been merely a question of explaining the Volta-force phenomenon, which, however interesting a matter, is not one of such paramount importance as to render other considerations secondary, it would have mattered little whether the localisation of the impressed force at the metallic junction were correct or not. But it involved the whole question of the relations of impressed force and potential, and the doing of work on or by the current. Sir W. Thomson's "Electrostatics and Magnetism," Maxwell's Treatise, and Professor Jenkin's "Electricity and Magnetism," al came out at about the same time. In the first, we had limited utterances on the subject, but Sir W. Thomson's paper on the "Size of Atoms" made his position clear. In the second, a remarkably complete theory of electricity was made indistinct in many places by the reservations concerning the potential of conductors, which was all the more surprising when it is remembered that Maxwell was no believer in the existence of the metallic-junction force. In the third was an account of previously unpublished experiments of Sir W. Thomson's, and a most decided statement of the theory, which asked us to believe that the situation of the electromotive force of a voltaic cell is not in the cell at all, but outside it, although admittedly the energy keeping up the current when the circuit is closed is derived from the chemical actions going on in the cell itself. This paradox must have intensely puzzled all readers, save those who brought a strong faith into operation in the course of their scientific studies.

Now, if we ignore the Volta-force experiments altogether, the general theory of impressed force, potential, and the taking in or giving out of energy by the current, is clear and explicit, contains no paradoxes, and is in harmony with general dynamical principles. Was it really worth while to upset the theory because some very curious experiments were difficult of explanation? Certainly theory must ultimately be made to agree with facts; but when *some few facts do not* apparently fit into a theory which

suits a much greater number of other facts, it becomes a question of balance of advantages whether it would be better to alter theoretical notions, or to leave the facts unexplained for the time, waiting for further information, or for new light on the question of fitting the facts into the theory. I think in the present instance, considering the extraordinary character of the alteration of theory, that the best course would have been to let the experiments wait for an explanation. All the more so on account of the long time it takes for views taught by the leaders of scientific thought, and accepted by their followers, to be eliminated should they turn out to be erroneous. Besides that, it was not exactly a case of altering a theory to suit facts, but rather to suit certain conclusions from facts, which might or might not be correct. The fact is, that the air outside zinc is at a different potential from that in the air outside copper when the two metals are in contact. The conclusion, quite distinct from the fact, is that the difference of potential is produced by an impressed force at the metallic junction, which makes the zinc and copper be at different potentials, with the further result that the electro-motive force of a battery is outside it. Nor can we clear up the matter by defining the potential of a conductor as the potential somewhere else (itself a paradox, which I learn with great surprise has always been taught by Sir W. Thomson), namely, in the air outside it. This makes the zinc and copper be at different potentials, *because* the air potentials are different, and necessitates an impressed force at the junction of the metals. It is the same case, slightly differently expressed.

Such was the extent of my respect, almost amounting to veneration, for Sir W. Thomson's opinions, on account of his invaluable labours in science, inexhaustible fertility, and immense go, that I made the most strenuous efforts to understand the incomprehensible, impelled thereto also by a feeling that it might be prejudice on my own part that made it incomprehensible. But, not possessing the faith of the early Fathers, I could not say, with St. Augustine, "Incomprehensible! therefore true;" or with St. Jerome, "I believe, because it is incredible." I therefore *finally gave it* up as a bad job, and evolved the views explained

in the article hereafter quoted out of my own inner consciousness, and of course felt immensely relieved in my mind at once. That the application of the well-known heats of combination method to find what the differences of potential should be for different pairs of metals gave figures very poorly agreeing with the observed differences of potential, I did not attach much importance to, considering how the state of the surfaces might alter results, and the unknown value of the thermo-electric force, which may not be so small at a metal-gas contact as at a metal-metal junction, and the unknown influence of the nitrogen in the air, which may not be wholly inert in the matter. That there was some sort of a general kind of agreement was quite as much as could be expected. Moreover, if there were absolute disagreement, it would not, in my opinion, shift the seat of the electro-motive forces from the air surfaces, but merely alter our views as to their cause.

I am inclined to confidently believe that the mere statement that the electro-motive force of a voltaic cell is *not* at the place where the energy transformation occurs which keeps up the current, is in itself sufficient, when rightly understood, to fully discredit any theory which necessitates that statement, when the matter is viewed generally from the modern dynamical standpoint. All the physical sciences are bound to become branches of dynamics in course of time, and anything contradicting the principles of dynamics should be unhesitatingly rejected. Without having made an exhaustive study of dynamics, I have yet managed to come to the conclusion that a force cannot act where it is not—meaning by acting, the doing of work. If the doing of work at one place involves the doing of work at another, the force doing the work at the second place is there, not at the first place. Of course there must be some connection between the two places whereby energy is transmitted between them, and wherever there is a transfer of energy going on there is force. All working forces involve transfer of energy, and the measure of a force, whether simple or generalised, is the amount transferred per unit change of the variable to which the force corresponds. In an isolated conservative system, all transfers of energy are internal, *and its total energy* remains constant. If such a system

receives energy, this involves impressed force, and a system communicating the energy. What to consider as impressed force depends upon how large we make our system, and is therefore considerably a matter of choice. Thus, if there be two systems, each conservative in the absence of the other, but with a transfer of energy between them, and therefore impressed force on either system when it is considered by itself, and if we include the two in a single system, their mutual forces cease to be impressed, becoming internal. These reminders are merely to illustrate force considered as impressed. Add to the above, that forces which always involve loss of energy from the system, never a gain—namely, forces of the frictional character depending on the velocities of the moving parts—are conveniently not reckoned as impressed, but as dissipative forces, reserving the term "impressed" for reversible actions, and we have a brief outline of the nature of the dynamical system which is represented, in a skeleton form, by the electro-magnetic equations.

In the absence of conducting matter, the system would be conservative and keep its energy unchanged in amount, or only lose it by setting bodies in motion. But disregard this, and let there be no relative motions of masses permitted. The presence of conductors introduces dissipative forces proportional to velocities (strength of current being a generalised velocity), and the energy tends to become used up through the Joule heat of currents in conductors; excepting that part may be locked up, as it were, as when insulated conductors are electrified, or when there is intrinsic magnetisation. It does not then waste itself; otherwise there is continual waste, which must be be compensated by impressed forces if the electric and magnetic energies are to be kept up. Their seat is in the ether. The actual constitution of the ether is unknown. It never can be *known*; but a constitution may be invented for it which shall admit of propagating heat and light and electro-magnetic disturbances to produce observed results. If the ether is made to propagate light (say, by vibrations), and will not propagate electro-magnetic disturbances, it cannot be the right construction, and another must be found. The transfer of energy in any conductor (isotropic) takes

place not with the current, but perpendicular thereto, as I showed in the *Electrician* for June 21, 1884, thus being delivered into a wire from the dielectric without. This does not hold good in the dielectric itself, where it is perpendicular to the electric force, or nearly parallel to the wire, into which it is continuously wasting itself, keeping up the conduction current. Both cases are included in the statement that the transfer of energy takes place perpendicular to the electric and to the magnetic *forces* of the system. (See *Electrician*, January 10, 1885, *et seq.*) That this is the correct statement is verified by its holding good in cases of strain or crystalline structure when the current or displacement is not parallel to the electric force, nor the magnetic induction to the magnetic force.

I have no doubt that some day a tolerably simple constitution of the ether will be invented, making it do anything reasonable that is required of it, which may in course of time come to be believed in as a reality, as light is now believed to be propagated by transverse displacements. But taking things as they are, with an unknown constitution of the ether, the electro-magnetic equations indicating a dynamical system, all actions on the system not included in the internal forces expressed by the equations must be impressed, and impressed somewhere. And impressed forces require to be very freely introduced, because, however complex the relations may appear to be in the electro-magnetic equations on first acquaintance, the essential parts are fundamentally very simple, and the infinitely numerous minor actions which are not accounted for can only be made to show themselves, electrically speaking, by means of the use of impressed forces on the one hand, and auxiliary investigations on the other. Now, although when to simplify matters we ignore details, as when we consider a linear electric circuit as a whole, its state being defined simply by the strength of current, we lose sight of the real energy transformations, and see only their result in the total, and cannot definitely say where energy is taken in or given out reversibly, yet if we, as we are justified in doing, take every portion of space by itself, we are bound at the same time to consider *every impressed force* to act upon the electro-magnetic

system at the very place where it is situated; and the sole conclusive direct evidence of there being an impressed force at a certain spot is in there being a reversible transformation of energy taking place there on the passage of an electric current, whilst we have indirect evidence, which may be equally conclusive, if we can show that there is no such action anywhere else.

If the Volta-force experiments were twenty times as difficult to explain as they have been considered to be, I do not see that there would be any sound reason for not concluding, or rather taking it for granted, quite apart from the Volta-force phenomena, that in a voltaic circuit, when we know that there is a transformation of energy going on, which accounts for the Joule heat in the circuit, the impressed force is exactly where an ignorant man would suppose it to be, namely, in the cell itself, although the exact distribution therein may be difficult to ascertain, owing to the complex nature of the actions. If it be not in the cell that energy is taken in by the current (to use an expression which should not be understood literally), but at an external junction, where there is no appreciable change occurring, it would follow that the energy of the chemical combination taking place in the cell did not result in an impressed force there, but first passed out of the battery to the junction, and was there taken in by the circuit. It must go to the junction first, to account for no change occurring there, and in the passage it must not act on the electro-magnetic medium, for that would mean impressed force in the cell. But no one would wish to believe in this roundabout process.

Maxwell's formulæ for the distribution of electric and magnetic energy in space may not be correct. But if others were substituted, giving the same results in the sum, and consistent with the laws of induction, we should still have to put the impressed force in the cell, unless we assume action at a distance, without the intervention of a medium, or save appearances by having two mediums.

I now interpolate my remarks on the Volta force and the voltaic cell, which appeared in the *Electrician* for February 2nd, *1884*. *It is* necessary to say, to account for the very short manner

in which the energy definition of impressed force is considered in the first part of the article, that it was one of a series, succeeding some on the subject of thermo-electricity, following therein Sir W. Thomson's beautiful thermo-dynamic theory, as applied to linear circuits. In this theory we have abundant illustration, in both the Peltier and the Thomson effects, of energy being taken in or given out by the current in any part of a circuit according as the current goes with or or against the impressed force. Some notes in square brackets are now added.

"Chemical Contact Force. (*Electrician*, Feb. 2, 1884.)

"We now approach one of the most interesting subjects in the whole of electrical science, on which there has been perhaps more debate than on any other of its branches. He is a learned man who is acquainted fully with all the details in the history of the matter." [This was prophetic of Professor Lodge's memoir.] "But one may not be therefore made wise. On the contrary, one may easily become confused in the attempt to reconcile the multitude of facts and hypotheses, especially as the observations are mostly only qualitative." [This was not prophetic.] "He may wish to obliterate all that has been done, and start afresh in the unbiassed state of mind accompanying perfect ignorance.

"Put any two metals in contact with one another, but otherwise insulated; they are said to acquire different potentials. That they are apparently at different potentials is made certain by the modern electro-metric measurements, using no finger contacts or multiplying machines. Thus, zinc and copper in contact apparently differ in their potentials by about ·75 volt. Professors Ayrton and Perry found this to be so constant that they used it as a standard of comparison in their observations on the apparent differences of potential of other metals in contact. It is proved that when zinc and copper are put in contact, the zinc becomes positively, the copper negatively electrified, and that they act inductively on other conductors, just as any two conductors similarly charged would.

"If we join the zinc and copper by a wire of some other metal (say, iron), *instead of making* immediate contact, just the same

thing happens; the difference of potential is ·75 volt as before. This applies to all pairs of metals, whence follows the 'summation law.' If metals A and B in contact apparently differ in their potentials by x volts, and A and C by y volts, then B and C will differ by $x - y$ volts.

"However, it is merely inferential that copper and zinc in contact really differ by ·75 volt. But assuming provisionally that such is the case, it follows that, since in a state of electrical equilibrium the whole of the zinc is at one potential, and the whole of the copper also" [thermo-electric or other impressed forces being ignored], "there is an electro-motive force of ·75 volt acting at their junction, from copper to zinc, this being required to balance the supposed difference of potential. If so, if we pass an electric current from any source across the junction, there will be, by elementary principles" [as exemplified in thermo-electricity], "a continuous absorption of energy when the current goes from copper to zinc, and generation in the converse case, amounting per second to ·75 × strength of current." [The word "generation" is here misapplied; if it were thermo-electric force, and for "energy" we read "heat," it would be all right. "Evolution" is preferable.] "Or, make a closed circuit of any number of metals and a battery; there will be similar absorptions and generations" [evolutions] "of energy at all the junctions, meaning by absorption that energy is taken in by the current from some source which, electrically speaking, may be called external, and by generation" [evolution] "that energy is given out by the current, or through its mediation.

"But there is no evidence of any such relatively enormous conversions of energy going on at metallic junctions. The known thermo-electric forces are of such inferior strength as to be almost of a different order of magnitude. The source of energy is heat, *i.e.*, the energy of molecular agitations. There may be other small conversions of energy, but certainly none able to account for an electro-motive force of ·75 volt between copper and zinc, or ·6 volt between zinc and iron.

"The thermo-electric forces being, then, so very small compared with the apparent contact forces now considered, we may

neglect them altogether, in order to save continual reference to them and small corrections. Copper and zinc, then, when placed in contact, are necessarily at One potential.

"It follows that if they were uncharged before being put in contact, and not in a field of electric force, they must have been at Different potentials. For, on contact, electricity passed from copper to zinc, reducing them to the same potential. But, having been, as stated, uncharged in the first place, and not in a field of force (or, say simply, neither showing any signs of electrification), the air being then all at one potential, and the potentials of the copper and zinc differing from one another, must be different from that of the air as thus defined:—Taking the air potential as zero, and that of the copper separately insulated as $(-x)$ volt, that of the zinc is $-(x + \cdot 75)$ volt. So far we do not know whether x is positive or negative, but we take it as positive here for convenience of statement. Thus a piece of uncharged zinc insulated in air has its potential $(x + \cdot 75)$ volt below that of the air, and a piece of uncharged copper insulated in air is also at a lower potential, but by a smaller amount, namely, x volts. This requires that there shall be, over the whole zinc surface, an electro-motive force of strength $x + \cdot 75$ volt, acting from zinc to air; and similarly over the whole copper surface an electro-motive force of strength x volt from copper to air.

"Electricity, in conductors, is subject to the same law of continuity as an incompressible liquid. There cannot be current entering a certain space without there being, at the same time, an equal current flowing out of that space. At the surface of conductors electricity was once supposed to accumulate. Maxwell extended the law of continuity to the surrounding dielectric. There is great advantage in this view in facilitating conceptions. We may imagine an incompressible liquid filling *all* space perfectly free to move by the slightest force in certain regions answering to pure conductors, with no tendency to return when displaced, but always meeting with resistance proportional to the velocity; also perfectly free to move in the rest of space answering to a pure dielectric, and without frictional resistance, but now only *elastically displaced*, so that there is a force of

reaction called into play proportional to the displacement, which will make the displacement subside when the force that produced it is removed." [By a "pure conductor" is meant one which is not also dielectric, or in which the displacement is not in any degree of the elastic character; and by a "pure dielectric," one in which the displacement is wholly elastic, or which has no conductivity.]

"Replace the material liquid by an imaginary something called electricity (not the electricity of the mathematical definition, but capable of becoming it by displacement)" [viz., at the bounding surface of a conductor and dielectric], "let it be free to move in conductors when acted upon by electro-motive force (answering to real force when the subject is a real fluid), but only capable of elastic displacement in the dielectric, and we may transfer results from one case to the other. We may remark, in passing, that the quasi fluid cannot be really matter, because that would require electro-motive force to be ordinary mechanical force.

"If to the surface enclosing a portion of the material liquid in which there is no reactive force, but outside which there is, we apply uniform normal pressure or tension, the liquid is not moved, because the forces balance, but the pressure within is increased or decreased by an amount equal to the applied surface pressure or tension.

"In the electric case, the uniform electro-motive force, $x + \cdot 75$, acting normally outward from a piece of zinc insulated in air, lowers its electric potential below that of the surrounding air by the amount $x + \cdot 75$, but cannot displace electricity. Similarly, the electro-motive force x, acting normally outward from the copper surface, lowers its potential below that of the air by the amount x. But the moment the copper and the zinc are touched, we substitute metal for air at the place of contact; the force $x + \cdot 75$ is removed from a portion of the zinc, and the force x from a portion of the copper surface; the differential force $\cdot 75$ volt acts; there is a current from copper to zinc, from zinc to air, and from air to copper" [*i.e.*, all round the circuit], "which is stopped by the force of reaction of the electric displacement in the dielectric. The zinc and copper are reduced to

the same potential; let this be y. Then, in the new state of equilibrium, the potential rises from y to $y + x + \cdot 75$ in passing from the zinc to the air, then falls continuously along the lines of electric displacement in the air till the air outside the copper surface is reached, when it is equal to $y + x$, and then falls by the amount x in passing into the copper, where it is y, the common copper and zinc potential.

"This may seem unnecessarily diffuse, but the importance of the subject and the difference of the above from views in general acceptance demand a somewhat amplified statement.

"The reason of the summation law readily follows. For let the zinc and copper, previously insulated, be joined by an iron wire. This, if insulated and free from charge, will have its potential lower than that of the air by $x + \cdot 60$ volt; or, $x + \cdot 60$ volt is the electro-motive force from iron to air. In contact with the copper only, when their potentials equalised, the field of force in the air would show a difference of potential along any line of force of $\cdot 60$ volt; and, in contact with the zinc only, of $\cdot 15$ volt, the iron being positively electrified in the first case, and negatively in the second. But when the iron wire is intermediate between the zinc and copper, the force $x + \cdot 60$ from iron to air, since it can now draw electricity both ways, from the copper and from the zinc, can have no influence in altering the difference of potential between the air just outside the zinc and just outside the copper, although altering the actual potentials relative to the original potential of the air. If z is the final potential of the three metals, those just outside the copper, the iron, and the zinc are $z + x$, $z + x + \cdot 60$, and $z + x + \cdot 75$, with a fall of $\cdot 75$ as before through the air from the zinc to the copper surfaces.

"It may be remarked that the field of force is perfectly determinate with any number of metals in contact, between each of which and the air there is a given electro-motive force. The bounding surface of the dielectric has then everywhere a given potential (+ a constant), and by Green's theorem this is sufficient to fully determine the distribution of force. Of course mathematical difficulties prevent the practical solution in general.

"In the above, we have, for simplicity, supposed the metals

to be pure and homogeneous, and to have clean surfaces. Some little difference is made when there are surface impurities. The nature of the effect may be readily seen. Start, for example, with a piece of absolutely pure zinc, and put a small particle of iron on its surface. The iron and zinc are at once reduced to the same potential, with positive electrification of the zinc and negative of the iron, and a fall of potential of ·15 volt through the air. Yet there will be no apparent electrification whatever, for the field of force can be only sensible quite close to the particle of iron, so that we cannot get at it. The air all round the zinc mass will be practically at one potential. If we enlarge the particle of iron, the field of force extends and becomes sensible at sensible distances, and so with further enlargement we can get sufficient separation of the parts of air at the extreme difference of potential to affect the electrometer inductively.

"Similarly, when there are, as in commercial zinc, innumerable foreign particles exposed to the air, side by side with the zinc and in contact with it, there are innumerable local fields of force quite close to the surface set up by the unequal electromotive forces. But at a sensible distance from the surface there can be no appreciable force; the air potential will be there unaffected, and the zinc will appear uncharged.

"Put this mass of impure zinc in contact with a mass of copper —it may be also impure; then, besides the complex local fields close to the surface, there is the extended one which can influence the electrometer. The difference of potential cannot be so great as with perfectly pure zinc and copper, the impurities acting to reduce it." [The effective metal-air electro-motive force, if there is average uniform distribution of impurities, may be taken as the mean value of the electro-motive force over a small area.]

"Now change the medium. Let zinc and copper be in contact, not in air, but in water, with a little acid to facilitate electrolysis; from being in a medium in which only electric displacement can happen, let the zinc and copper be wholly immersed in an electrolyte. The surface electro-motive forces are now probably not the same—it is very unlikely that they *should be*—but there they are. Instead of their producing a

mere momentary current, we have now a continuous current from zinc to liquid, liquid to copper, copper to zinc. The two metals are not now exactly at one potential, owing to the current, but practically all the fall of potential is in the liquid. The lines of force, which are of course also the lines of flow of the current, are, when the sides of the vessel containing the liquid are sufficiently remote, distributed in the same manner as the lines of force in the corresponding case with air as the medium, though of course they become considerably altered if the vessel is small, the current being forced to be tangential at its sides.

"The local superficial fields of force have now great importance, for there are naturally local currents to correspond between the zinc and its impurities, with consequent waste of energy—waste, in not being externally available. This is the same when the zinc is alone in the liquid. The purer the zinc, the more slowly is it burnt in acid. Absolutely pure untarnished zinc would last for ever, owing to the balance of forces; but the least impurity getting on the surface would start galvanic action." [And I suppose any want of uniformity of temperature or strength of solution round the imagined pure zinc would suffice to start some small amount of chemical action.]

"If a copper wire joins the zinc and copper, all being still wholly immersed, circumstances are not materially altered; the current goes from the zinc to the copper (say plates, now), and also to the copper wire through the liquid, and back through the wire; the current in the wire, however, is not everywhere of the same strength. But lift the wire out of the liquid, together with that portion of the zinc plate to which it is attached, and the whole current (not counting the local currents) returns by the wire outside the vessel, and we have a full-blown galvanic cell.

"The new electro-motive forces introduced by the new contacts—viz., between the zinc and air, and between the copper and air—do not in any way alter the integral electro-motive force in the circuit, nor can any difference of potential between the liquid and the air. The metals in connection may be nearly at one potential, or may differ by nearly the full electro-motive force of the cell, *according to the resistance* of the external wire. There

is a large rise at the zinc-liquid surface, and a fall of much smaller amount at the liquid-copper surface, the excess of the rise at the zinc over the fall at the copper being equal to the available electro-motive force of the cell. But in other galvanic arrangements, as when there are two fluids, the electro-motive forces and changes of potential become more complex.

"The absorption of energy is at the zinc surface where the current goes with the electro-motive force there. The evolution is at the copper surface where the current goes against the electro-motive force there. The excess of the former over the latter becomes heat in the circuit.

"At the zinc surface we know that there is oxidation of zinc, and the supply of energy is readily accounted for. The heat which would have been produced locally if the zinc were burnt in oxygen now turns up in all parts of the circuit, through the intervention of the unknown electric agency, and the artificial disposition of conductors and insulators we have made.

"The evolution of energy at the copper surface is more obscure. There is a local development of heat independent of the frictional heat in the circuit. The heating of galvanic batteries has not been fully investigated.

"Regarding the cause of the electro-motive forces, next to nothing is known. Separated zinc and oxygen have potential energy, they tend to unite, and in the act of union a store of energy is set free. At the same time there is electro-motive force from the zinc to the oxidising agent. But why zinc and oxygen should unite, or why electro-motive force should accompany the action, I have not come across any intelligible explanation, and I do not expect it." [Professor Lodge's memoir contains no information on these important questions.]

"But the known transformation of energy taking place at the zinc surface in our galvanic cell, together with the similarity of electrical conditions, enables us to conclude with a tolerable amount of certainty that the source of the electrostatic energy which is set up when zinc and copper are put in contact in air is oxidation of the zinc. The amount of oxidation is of course very *small—infinitely* unrecognisable." [This is an exaggeration.]

"This will be evident on remembering what a large quantity of electricity must pass before any visible consumption of zinc takes place in the cell, or even before enough is consumed to be detectable by the most delicate chemical balance. In the air case the action is stopped in its very birth by the elastic reaction of the electric mechanism. The facts observed long ago by Sir W. Thomson confirm this conclusion regarding oxidation. The difference of potential is greatest when the zinc surface is clean—that is, in the best state for oxidation—and when the copper surface is already oxidised, and therefore in its worst state, amounting then to about 1·1 volt, instead of only ·75 volt."

After making the above statements I proceeded to make some more in the two following numbers of the series (March 1 and 31, 1884), on matters connected with contact force, in connection with the layers of electricity supposed, originally by Helmholtz, to accompany impressed forces, as well as on the relations of potential and impressed force. They are, together, too long for quoting here, but I think their perusal might be useful to some who have not already made up their minds on these matters. But I may quote an extract immediately illustrating the application of contact layers to the Volta-force experiments. The surface-impressed force cannot be regarded as acting at a mathematical surface, but must extend through some small depth, perhaps excessively small, so that we have a thin layer of electromotive force, the force acting straight across.

"On one side of the stratum, that *to* which the force acts, there is supposed to be an accumulation of free positive electricity, and on the other side an accumulation of negative, which produces an electric field resembling that of a charged air condenser, whose force wholly cancels the contact force when there is equilibrium, and partly cancels it when there is current. The surface density of the accumulations must depend upon the thickness of the stratum, being great when it is small, and conversely. If t be the thickness of the stratum, and E the difference of potential, the electric force *per unit length* $= \mathrm{E}/t$. Hence, by the definition

of the unit of electricity applied to a surface distribution, the surface density is $\sigma = E/4\pi t$.

"These electric layers are brought into great prominence when the electro-motive force acts all over a closed surface—for example, when zinc is immersed in air. To make the force quite uniform, we may imagine electro-motive force to be applied at those places where it is supported equal to that acting from the zinc to the air. The electric layers will now form a pair of closed surfaces, very close together, wholly surrounding the conductor, the positive layer outside, the negative within. This combination we may call a closed electric shell, from its obvious similarity to the closed magnetic shell which appears in the theory of magnetism. The electric force of the shell is wholly self-contained—that is, it is situated between the two layers of electricity, directed straight across the stratum from one to the other, with no electric force either within the inner or outside the outer layer. The potentials are uniform inside and outside the shell, but differ by the amount E, if E is the electro-motive force from the zinc to the air. For if a unit charge of electricity be carried from inside the conductor to the external air, it will travel against the electric force of the accumulations, and work must be done on the charge to the amount force × distance or $E/t \times t = E$, which is therefore the excess of the outer above the inner potential. In this we consider the electric force due to the layers alone, for the resultant force being nothing, no work would really have to be done.

"Comparing with a closed magnetic shell, if its positive side be the outer, the outer magnetic potential exceeds the inner by 4π × strength of the shell. This conforms to the above, remembering that the 'strength' of a simple magnetic shell is defined to be the magnetic moment of unit of area, and is therefore = surface density of magnetism × thickness of shell. We might similarly define the strength of the hypothetical electric shell, but it is not worth while doing so, as the amount of the difference of potential sufficiently settles it.

"Now these electric layers, if they existed, would be wholly independent of any real charge that we might communicate to the *conductor*. Say, for instance, we charge it by contact with some

other metal. This will not alter the electric layers in any way." [There is a very small alteration due to the depth not being infinitely small.] "If they were there before, they are there still, for there is still the same difference of potential at the surface" [very nearly]. "The real charge, being connected by lines of force through the air with other conductors, is of course recognisable, but no tests can be applied to the associated layers. Perhaps it would be most reasonable, as it is simplest, to put the real charge outside the outer layer, rather than within the inner, or between them, if we must have the electric layers. But although this extraordinary complication of the surface conditions by the presence of the layers may be used as an argument against their existence, still such argument would be no proof that they do not, or that they cannot exist."

I may add to this extract that I have no faith whatever in the existence of these layers of electricity, but must refer to the articles for particulars. I am sorry not to have Professor Lodge with me in this part of the matter, as the wide publicity he gives to his views adds force to his powerful advocacy. I may add that layers of electricity in connection with impressed force are apparently widely believed in, even by followers of Maxwell; possibly in the last case because he was not always true to his principles, putting, for example, free electricity in the interior of conductors, in defiance of his law of continuity of the current. There may be free electricity in conducting matter if it be dielectric as well, and heterogeneous (and all conductors may support elastic displacement to some extent), but its distribution will not by any means be the same as that supposed.

Practically these things matter very little, but theoretically they matter a great deal, as it is important to have theory as definite as possible, as well as consistent with itself.

To return to the Volta force. Although we are agreed on all essential points, I cannot very well follow Professor Lodge's straining atoms, nor see their utility in the argument. This is because I know nothing about atoms. I cannot think that he knows much more. But, on the other hand, we do know something, however little, about the law of the electric current, that

it "flows" in closed circuits, as if it were an incompressible liquid, and that in consequence there can be no current leaving a conductor, or displacement, if the impressed force act equally all round it, owing to the balance of the electro-motive force. The real interpretation of the quasi-incompressibility we do not know, but admitting it, there is no difficulty. We also know that chemical affinity, or tendency to chemical combination, is measureable in terms of electro-motive force, so that, as there is chemical affinity between oxygen and zinc, and air contains oxygen, there must (irrespective of the argument based upon the absence of reversible effects at metal junctions) be an outward acting electro-motive force all over zinc alone in air, and therefore no current or displacement, as above. Electrically expressed, it is intelligible.

But when we, after Professor Lodge, put the electrical conditions in the background, and consider the oxygen atoms round a piece of zinc all straining at and trying to combine with it, we may well ask, why don't they do it? All the more should they do it if they are straining *all* round, unless they get wedged together, so that it is necessary to remove some of them to let the rest go nearer, possibly quite close enough to combine with the zinc. No, without such an irrational supposition, we must fall back upon the electrical argument, and then we do not want the imagery of straining atoms, for it is intelligible by itself. If it were thermo-electric force, for example, it would be necessary to recast the imagery, whilst the electrical argument remains the same.

I am not objecting to the use of the imagination. That would be absurd; for most scientific progress is accomplished by the free use of the imagination (though not after the manner of professional poets and artists when they touch upon scientific questions). But when one, by the use of the imagination, has got to a definite result, and then sees a stricter way of getting it, it is perhaps as well to shift the ladder, if not to kick it down. For I find that practically, in reading scientific papers, in which fanciful arguments are much used, it gives one great trouble to *eliminate the* fancy and get at the real argument. Nothing is

more useful than to be able to distinctly separate what one knows from what one only supposes.

To return again to the Volta force. When we desire to go further and inquire what is the exact nature of the energy transformation that takes place when the electro-motive force is removed from a part of the zinc surface by contact, say, with copper, thus causing a current to pass in the circuit copper-zinc-air-copper, setting up a state of electric displacement in the dielectric with a certain definite amount of potential energy depending on the capacity of the condenser which the arrangement forms, we enter a very difficult and speculative matter, whose solution one way or another will not alter the preceding. Take two flat plates of zinc and copper, for example, and put them close together, not touching. The moment they are connected the condenser becomes charged. Let U be the potential energy of displacement. This equals $\frac{1}{2}$ E Q, if E be the difference of potential and Q the charge, which again = E S, if S be the capacity of the condenser. Now at the same time as the potential energy, U, was set up, an equal amount of Joule heat was generated (an example of a general law, concerning which those interested in the matter may be referred to an article by me now awaiting space in a forthcoming number of the *Electrician*). Thus we have 2 U of work done. Besides this, if there be a copper-air force of strength x, so that x + E is the zinc-air force, and if this force was present when the current passed, there must have been x Q of work done at the copper surface. Thus

$$
\begin{aligned}
(E + x)\,Q &= \text{work done by the zinc-air force,}\\
&= \tfrac{1}{2}\,E\,Q, \text{ electrostatic energy,}\\
&\quad + \tfrac{1}{2}\,E\,Q, \text{ Joule heat,}\\
&\quad + x\,Q, \text{ work done at the copper-air surface.}
\end{aligned}
$$

Now, as there is a loss of energy $\frac{1}{2}$ E Q necessarily, it cannot be a case of mere pulling backwards and forwards of atoms, with conservation of energy.

But irrespective of this loss, I am strongly of opinion that there is not a mere yielding to a tendency to chemical combination, but an actual *combination* when the current passes—an actual

minute amount of oxidation of the zinc, as expressed in the article above quoted.

To illustrate, start with a conductively closed voltaic circuit. It is admitted that the steady generation of heat in the circuit is derived from the energy of chemical combination in the cell. For we know that there is combination, and that the heat of combination is of the right amount. Now suppose we suddenly insert a very large condenser in the circuit, so that the current is still kept up, although the circuit is interrupted in the common language, the energy being now delivered into the dielectric of the condenser, as well as into the conductor, though at a decreasing rate, owing to the elastic displacement set up putting a gradual stop to it, till finally the current becomes insensible. Will it not be granted that during the whole time the decreasing current passed, the energy was derived from chemical combination, even down to the last dregs of current, including the weak current of apparent absorption? and that the same applies when we decrease the capacity of the condenser till at last we come down to a voltaic cell with two bits of copper wire attached to the plates? If not, where shall we draw the line between chemical combination on the passage of a current, the electro-motive force being measurable by the heat of combination, and merely a yielding to the tendency, with its necessary indefiniteness? And why should we draw any line?

Or, in another form, let the copper be in the cell first, then put in the zinc with attached wire, thus passing a minute quantity of electricity. Is it not due to chemical combination? If not, we are placed in the gratuitously difficult position that we must pass a certain quantity of electricity before any combination occurs at all. Now I do not want to assert that electrical laws holding good on a large scale necessarily continue true in the same form on all smaller scales, however small. This would bring us to fractional parts of an atom at last. But in the experiment just mentioned, however feeble the effects may be, they must be still far elevated above atomic fractions, and I therefore see no reason for drawing the line.

Nor do I see any reason for drawing the line in the

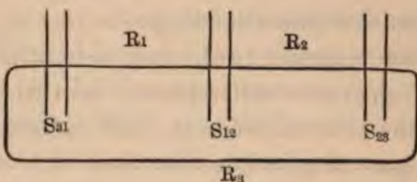
corresponding air experiment with copper and zinc. The air tends to oxidise the zinc, but cannot when the zinc is alone, for reasons intelligible when electrically expressed, otherwise indefinite, as before mentioned, there being a balance of electro-motive force. But destroy this balance, removing the cause that prevented combination occurring, by putting copper in contact with the zinc. I can see no reason why it should not occur, lasting till there is again electrical equilibrium. That the air battery and the voltaic cell are not exactly alike, air not being an electrolyte transporting ions delivering up their imaginary charges, I do not consider any objection. We do not need transport of matter. The dielectric carries the current, according to Maxwell's theory, and that is what is required. The action at the copper-air surface is of course very obscure, necessarily more so than in the case of the voltaic cell, where it is obscure enough.

If we examine according to the law of induction what occurs when zinc and copper are connected, we find that the disturbance commences at the place of contact. This is, however, merely an example of the general principle that when we alter the electrical conditions anywhere, causing a previously steady state to be upset, the disturbance commences at the place where the alteration was made. Thus, if a battery be on at one end of a submarine cable, with its farther end insulated, and we then put the latter to earth, the "signal" will travel *to* the battery. In the Volta-force case, suppose that, on making contact, we instantaneously remove the air from a circular patch of zinc, the copper touching it all over the patch. Then the disturbance will start from the circle bounding the patch, the zinc-copper-air line. This line is the first line of magnetic force. As it comes on, electric current flows round this line, infinitely close at first, partly through the metal and partly through the air, and the magnetic force spreads laterally, and with it the electric current. But practically we could not instantaneously make contact over such a patch, but would commence contact nearly at a point, which would be the first origin of the disturbance.

Now we know that some pressure is required to make a good contact. When *it is light*, it is microphonic, has considerable

resistance, which is variable with the strength of current (when in a closed circuit with a battery), and there is really air between the supposed touching surfaces, through which the current passes. Have any experiments been made to ascertain the influence of such microphonic contacts on the magnitude of the Volta-force difference of potential? Does the difference of potential come on gradually with increasing pressure, or does it come on all at once at a certain stage in the operation? Further, I would ask, has mechanical work done in making the contact any concern in the matter, as in squeezing out air?

I conclude, for the reason given at the commencement of this paper, with the theory of impressed force in condenser circuits. Let there be three condensers, which is enough for generality.



Let the positive direction in the circuit be clockwise, right to left below, left to right above. The three conductors have resistances R_1, R_2, R_3, and the capacities of the condensers are S_{31}, S_{12}, S_{23}, the numbers showing between which conductors they are placed.

Let the potentials on the left sides of the condensers be P_{31}, P_{12}, P_{23}, and the falls of potential in passing through them be p_{31}, p_{12}, p_{23}. Let E_1, E_2, E_3 be the impressed forces in the conductors, and e_{31}, e_{12}, e_{23} those in the condensers. E_1 is the total impressed force in R_1, anyhow distributed; strictly it is the total of the impressed force resolved parallel to the length of the wire, the impressed force at any section being taken to be the mean value of the force over the section, so far as the linear circuit is concerned. Similar remarks apply to the impressed forces in the condensers, where, owing to the large cross-sections, they are more obviously required.

Let C be the current in the circuit and Q the common charge,

supposing that we start with the condensers uncharged, so that Q is the integral displacement. Then, by Ohm's law applied to each conductor, we have

$$\left.\begin{array}{l} P_{31} - p_{31} - P_{12} + E_1 = R_1 C \\ P_{12} - p_{12} - P_{23} + E_2 = R_2 C \\ P_{23} - p_{23} - P_{31} + E_3 = R_3 C \end{array}\right\} \quad \ldots \quad \ldots \quad (1)$$

Adding these, we find

$$E - \Sigma p = R\,C \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (2)$$

where R is is the sum of the resistances, Σp the sum of the falls of potential through the dielectrics, and E the sum of the impressed forces in the conductors. From this we see that when the steady state of no current is reached, and $C = 0$,

$$E = \Sigma p,$$

or it is the impressed forces in the conductors only, irrespective of what the forces may be in the condensers, that is opposed by the sum of the condenser differences of potential. If no total impressed force in the conductors, the sum of the falls of potential is zero. I have forgotten to mention that the E's and e's include the E.M.F.'s of induction, though, strictly speaking, we should confine the term "impressed force" to a force which is neither that of induction nor derived from difference of potential.

Now, also,

$$Q = S_{31}(e_{31} + p_{31}) = S_{12}(e_{12} + p_{12}) = S_{23}(e_{23} + p_{23}) \quad (3)$$

is the common charge, and

$$C = \frac{dQ}{dt} = S_{31}\left(\frac{de_{12}}{dt} + \frac{dp_{12}}{dt}\right) = \text{etc.} \quad \ldots \quad (4)$$

or, when the impressed condenser forces are steady,

$$C = \frac{dQ}{dt} = S_{31}\frac{dp_{31}}{dt} = \text{etc.}$$

Put (3) in (2); then

$$R\,C = E - \frac{Q - S_{31}e_{31}}{S_{31}} - \text{etc.}$$

Or,

$$R\,C = E + e - \frac{Q}{S} \quad \ldots \quad \ldots \quad \ldots \quad (5)$$

where e = sum *of impressed* forces in the condensers, and S is

the reciprocal of the sum of the reciprocals of their capacities, or the capacity of the three in sequence.

Or,
$$C = \frac{E + e - Q/S}{R}$$

$E + e$ is the total impressed force (this must include the total electro-motive force of induction in the circuit), and Q/S the back force of elastic displacement.

And when the steady state is reached, $C = 0$, and
$$Q = S(E + e),$$
or the common charge = total capacity × total impressed force (which now of course contains no electro-motive force of induction).

Q being known, by (3),
$$p_{12} = Q/S_{12} - e_{12}, \text{ etc.},$$
give the falls of potential in the condensers, and (1) with $C = 0$ gives the falls in the conductors. To go into the manner of fall, the exact distribution of impressed force is required to be known. It must exactly cancel the impressed force in the conductors; and in the condensers the impressed force per unit length + the fall of potential per unit length $= \frac{4\pi}{c}$ × displacement per unit area, c being the specific capacity. The displacement per unit area is of course Q ÷ area of the condenser considered.

The absolute value of the potential anywhere is left arbitrary, and it has no absolute value, not signifying any physical state, which is signified by the electric force. The potential is a quantity that by its variations gives an auxiliary distribution of force, which, together with the impressed force, makes up a complete system of force to suit the continuity of electricity, Ohm's law and Maxwell's law to match. There is no electrification *in* the conductors, or *in* the dielectrics, however the impressed force varies in distribution. The only electrification is at the boundaries between the dielectrics where the displacement is elastic, and the conductors, where it is not. But we need not be misled by the term displacement to think there is anything displaced in the direction of displacement.

If a condenser contain no impressed force, its fall of potential is proportional to its inductive resistance (reciprocal of capacity);

if it contain impressed force, it is the sum of the impressed force and the difference of potential that is proportional to the inductive resistance.

We may write

$$p_{12} = S\left(\frac{E + e}{S_{12}} - \frac{e_{12}}{S}\right);$$

so, if

$$S : S_{12} = e_{12} : e,$$

or the impressed force in any condenser is the same fraction of the total impressed force in all the condensers as the inductive resistance of the condenser is of the total inductive resistance, we have

$$p_{12} = \frac{E\,S}{S_{12}},$$

the fall of potential now depending on the impressed force in the conductors, being the same fraction as before mentioned of the total conductor impressed force. And if $E = 0$, then $p_1 = 0$; that is, if the impressed forces be in the condensers only, and proportional in each to its inductive resistance, there is no difference of potential in any part of the system if they be uniformly spread in each condenser; otherwise the conductors are all at the same potential, and the condensers appear uncharged, but there are variations in the condensers themselves.

By (2) and (3) the differential equation of the current is

$$\frac{C}{S} + R\frac{dC}{dt} = \frac{dE}{dt} + \frac{de}{dt},$$

or, adding the electro-motive force of induction separately, which is

$$-L\frac{dC}{dt},$$

if L be the co-efficient of self-induction of the circuit,

$$\frac{C}{S} + R\frac{dC}{dt} + L\frac{d^2C}{dt^2} = \frac{d(E + e)}{dt},$$

which, save in the presence of e, does not differ from the ordinary case of a coil and condenser. It is in the potential details that the cases of impressed force in the conductors and in the dielectrics require to be distinguished.

In the Volta-force experiment (say, copper and zinc), we have a feeble thermo-electric force at the metallic junction, feeble thermo-electric forces in the zinc and copper if temperature

varies, and two big forces, one of $x + \cdot 8$ volt, say, in a thin layer of the dielectric next the zinc surface, and one of x volt similarly at the other end of the dielectric. On charging, the rise of potential is very nearly as much at the zinc layer, with very nearly as much fall as before at the copper layer.

The theory of impressed force and potential in a dielectric is curiously illustrated by the phenomenon of absorption. The electric elasticity is not perfect; under the action of the stress the dielectric slowly yields, and with it a part of the displacement set up by an impressed force outside ceases to be of the elastic character, becoming intrinsic, and the difference of potential falls, requiring more current to enter to keep up the difference of potential.

The first discharge, like the first charge, is of elastic displacement. What is left, which shows no signs of being there at all, was elastic, but is no longer so. We may regard it as being kept up by uniformly distributed impressed force in the dielectric itself, arising from altered state of the dielectric produced by loss of elasticity. In time it recovers itself, or the impressed force is taken off, when the residual charge shows itself by the difference of potential it can now produce.

To really discharge the condenser at once, we must apply, after the first discharge, an opposite impressed force of the right amount, of course apparently charging the condenser oppositely to before. Leave it to itself, disconnected, and the apparent charge will gradually disappear.

Residual magnetisation in soft iron is somewhat analogous, but the effect is of far greater magnitude, and there is permanent set as well, which becomes predominant in intrinsic steel magnets. But we can set up permanent set of displacement also in a dielectric, as by passing a current through warm glass and then cooling it. It is then like a permanent magnet.

If we had conductors for magnetic induction (analogous to electric conductors), we, by magnetising a plate of iron setting up residual magnetisation, could apparently discharge it so as to show no force outside. It would then be like the charged condenser *(in which* "absorption" has occurred) after its apparent *discharge.*

The One Hundred and Forty-fifth Ordinary Meeting of the Society was held at the Institute of Civil Engineers, 27, Great George Street, Westminster, on Thursday, the 14th May, 1885—C. E. Spagnoletti, Esq., M. Inst. C.E., President, in the Chair.

The minutes of the last meeting were read by the Secretary, and confirmed.

The President then called upon Professor Andrew Jamieson to read his paper on

ELECTRICAL DEFINITIONS, NOMENCLATURE, AND NOTATION.

By Professor Andrew Jamieson, C.E., F.R.S.E., Member.

Principal, College of Science and Arts, Glasgow.

With the rapid progress that has lately been made in electrical science and its applications, there has sprung up a new and fast-increasing class of practical electricians. These, partly from necessity and partly from well-meant respect, have adopted and applied the old terms and expressions which appeared suitable to their predecessors, as well as coined not a few new ones, until now their vocabulary is in considerable confusion, and, as all must admit, requires sifting and reform.

Nothing is more tantalising and perplexing than the different modes of expression and symbols used by different authors, and sometimes by the same author, to explain and interpret one and the same thing or result. All this might be avoided if an international system of definitions, nomenclature, and notation was agreed upon and legalised. The rapidity with which the new definitions of the ohm, ampère, and volt (issued and legalised last spring at Paris by the International Congress of Electricians) were universally adopted, shows this. These definitions should be still further extended to other electrical units. They should embrace a suitable system of notation, whereby electricians could represent in symbols and letters, terms, expressions, and formulæ of common occurrence, in a

similar manner to that adopted by chemists in connection with chemical elements and their combinations. Last session the author promised a communication to the Society on this subject, and, being again reminded by the Secretary of his unfulfilled promise, he now submits a few of the more apparent instances where ambiguity or want of uniformity exists, with suggestions, in the hope that a discussion may follow, and that a committee of this Society may be formed to consider and draw up a series of definitions, nomenclature, and notation that would be generally acceptable. The proposed committee might then confer with the French Committee, also with a similar committee appointed by the British Association, and, finally, this important question should be referred to the International Congress of Electricians, in order that they may legalise and issue their decisions in a similar manner to that adopted by them in the case of the ohm, the ampère, and the volt. Undoubtedly, if such a course were adopted, most beneficial results would accrue to all concerned.

Only last November, M. Hospitalier brought this subject prominently before the International Society of Electricians at Paris, and strongly advocated an investigation, so that you will no doubt have their support and concurrence.*

---

* Communication faite à la Société Internationale des Electriciens, le 5 Novembre, 1884, par M. E. Hospitalier sur L'Unité de Définitions, Conventions, Notations, et Symboles Électriques (*vide L'Electricien*, 15 Décembre, 1884).

"Sur la proposition du Président, l'assemblée décide qu'une Commission spéciale sera nommée à l'effet de rechercher les meilleures méthodes à adopter pour les notations électriques et de codifier ces notations.

"M. le Président propose, au nom du Bureau, d'appeler à faire partie de la Commission des notations électriques.

"MM. Ed. Becquerel.	MM. H. Becquerel.
„ E. E. Blavier.	„ G. Cabanellas.
„ Marié-Davy.	„ J. Carpentier.
„ Tresca.	„ Gauthier-Villars.
„ Maurice Lévy.	„ E. Hospitalier.
„ G. Lippmann.	„ D. Monnier.
„ Félix Lucas.	„ D. Napoli.
„ Mercadier.	„ Pollard.
„ De Meritens.	„ J. Raynaud.

M. V. Williot.

"*L'assemblée* adopte cette liste à l'unanimité."

Examples.—1. At the very outset students are perplexed by such different terms as "Ordinary* or Static or Frictional or High-tension Electricity." One author will tell his readers or students: "For a long time the name Frictional Electricity was given to a group of phenomena produced by electrical charges. This is an improper expression, because friction is only one means for producing electrical charges."† Another says: "Static Electricity is, however, a misnomer: it has no existence: all the phenomena are due to static strains, but there is always a gradual loss called leakage, which is, however, the current due to the actual conductivity of all circuits, and every motion set up by so-called static electricity implies a transfer of energy and action occurring in a field of force set up in the form of strains in the particular inductive circuit in which the motions occur."‡ A third objects to the word "tension" in respect to electricity, and points out that "all the phenomena observable in connection with so-called High-tension Electricity may be produced by electricity drawn from batteries or dynamos if the electro-motive force or difference of potential is sufficiently increased." Would not the term "Electrostatics" be more suitable and comprehensive? §

2. The old nomenclature "vitreous" and "resinous," as applied to substances which when rubbed by certain other substances produce opposite electrical properties, and the scholastic one and two fluid theories based upon these effects, should be discarded for the more comprehensive modern theory of electric polarity of molecules or contiguous particles, expressed by "positive" and "negative," or by the algebraical signs (+) and (−).

3. "Electrics," "dielectrics," "nonconductors," "insulators," and "isolators" are terms used by different writers to express a condition or behaviour of certain materials with respect to electricity, in contradistinction to the terms "nonelectrics" or "conductors" as applied to other materials. The words "electrics," "nonconductors," and "nonelectrics" are, strictly speaking,

* The term used by Faraday. (See "Experimental Researches," by Michael Faraday, p. 82, art. 264.)

† "Electrician's Pocket-book," by E. Hospitalier, p. 5.

‡ "Electricity," *second edition*, by Sprague, p. 6, art. 20.

§ "*Electricity and Magnetism*," by Clerk Maxwell, Vol. I., Part I.

meaningless, because all materials may be termed electrics and all conductors, only differing in degree. The words "isolators" and "isolation" (from the French verb *isoler*, to isolate or separate) should give way to "insulators" and "insulation" as applied to substances which offer a comparatively greater resistance to electricity than semi or good conductors used in connection with the apparatus being referred to at the time.

The term "dielectric" * was first used by Faraday on finding that conduction was effected by induction (of polarity from molecule to molecule), and is generally employed by practical electricians when speaking of the inductive capacity of the insulating material surrounding the conductor of leading wires or submarine cables, or that placed between the plates of a condenser. In this sense, viz., of a body transmitting electric induction, or capable of undergoing electric stress, and retaining the stressed condition, it is a very appropriate term to use.

4. The term "accumulator" is the name given in several text-books to apparatus, such as the Leyden jar or condenser, for receiving and retaining quantities of electricity, but has been lately inappropriately applied to secondary batteries, which do not accumulate electricity.

5. "Cascade," as applied to Leyden jars, should give way to "series."

6. "Tension,"† "potential," and "electro-motive force," are terms which, when variously and indiscriminately applied, have given rise to considerable confusion, and a great deal of writing in trying to define them. If we consider "tension" as simply the stress put upon the circuit by the electro-motive force, and not in the sense that it used to be employed (for example: "Join up a battery or set of condensers for tension"), it might do very well if kept in its place; but it can easily be dispensed with. "Potential" is a word that has also given great trouble. We find

---

* Faraday's "Experimental Researches," p. 364. "I use the word *dielectric* to express that substance through or across which the electric forces are acting." (See also pp. 537, 538.)

† For a good definition of these terms, see "Electricity and Magnetism," *by Clerk Maxwell*, Vol. I., p. 49.

in "Sprague's Electricity"* no less than three pages devoted to an explanation of the different ways in which the words "tension" and "potential" are employed. Clerk Maxwell said: "The theory of electrostatics is greatly simplified by the introduction of this new conception of potential." "As soon as we pass from electrostatics to other departments of electrical science, we find that the conception of potential is no longer available, except when used in a restricted sense and under carefully-defined conditions." "In other parts of electrical science we have to deal with electro-motive force in cases where 'potential' and consequential 'potential difference' are words without meaning." Professor Fleeming Jenkin, in his well-known text-book on electricity and magnetism, devotes twenty-six pages to "potential," and defines "unit difference of potential or electro-motive force in electrostatic measure to exist between two points when the unit quantity of electricity in passing from one to the other will do the unit amount of work." "The property of producing a difference of potential may be said to be due to a peculiar force, to which force the name *electro-motive force* is given." "The words *electro-motive force* and *difference of potential* are used frequently one for the other, but they are not, strictly speaking, identical." "Electro-motive force is the more general term of the two, and includes difference of potential as one of its forms." "Potential" might well be reserved for electrostatics, and "electro-motive force" for electrokinematics, or current electricity, and thus prevent confusion. The word "electric-pressure" has come into vogue lately, and strongly appeals to those of a mechanical turn of mind, seeing that the hydraulic simile of "head" or "pressure" is often brought forward to assist in explaining the terms "potential" and "electro-motive force."

7. In magnetism we find the same want of uniformity exists. Take the case of a freely-suspended magnetised needle. The pole which turns towards the geographical north is variously called the "austral pole," "north pole," "north-seeking pole," "marked pole," and is painted red by Sir William Thomson, while Sir

---

* *1884* edition, pp. 58-62.

George Airy, Professor Guthrie, and others, paint it blue. It is sometimes indicated by French makers by the letter *A*, and by British by the letter *N*. The pole which turns towards the geographical south is correspondingly called the "boreal pole," "south pole," "south-seeking pole," "non-marked pole," painted blue by Thomson, and red by Airy, Guthrie, and others, and indicated by the letters *B* or *S*. Such is the general doubt and diversity in regard to the nomenclature on this subject, that each author on magnetism considers it necessary to state at the outset which term and symbol he intends to apply. If once for all the pole which turns towards the north was termed the "*north* pole," painted blue, and indicated by the letter *N*, and the opposite pole was termed the "*south* pole," painted red, and indicated by the letter *S*, much vexation would be saved. The French terms "austral" and "boreal," with letters *A* and *B*, should be obliterated. In this way the earth would have a uniformly recognised polarity, which would of course be opposite to that of the magnetised needle—in other words, the true north pole of the earth would be that situated near the geographical south pole.*

8. Sailors and some writers on the mariner's compass call the angle which the magnetic meridian makes with the geographical meridian, the "variation" of the compass, while electricians call it the "declination." Variation is, properly speaking, the hourly, diurnal, annual, or secular changes which occur in the value of the elements of terrestrial magnetism. This leads to great confusion and argument between the electricians and the officers of a telegraph steamer. The declination for each place is marked on the Admiralty charts. Sailors also speak of the "deviation" of a compass, meaning by that the local error due to the resultant of the quadrantal, semicircular, and heeling errors. It would be far better if they simply spoke of the "compass-error," or angle which the meridian of their compass-needle makes with the true north and south magnetic bearings.

9. When we come to electricity generated by batteries, we

* Sir Wm. Thomson calls the magnetic pole of the earth, situated near the geographical north, the "north pole," and the end of the magnetised needle *which points* towards it, the "*true south pole*" of the needle, and paints it red.

find the expressions "galvanism," "voltaic electricity," "dynamic electricity," "electrokinetics," "current electricity,"* etc., according to the fancy of the writer or speaker. Surely one name might suffice; and certainly the older terms "galvanism" and "voltaic electricity" might well be left to the past. The simple term "current electricity" seems to commend itself, as most of the effects in connection with this branch of the subject have reference to electricity as if it was in motion or distributing itself over a conductor.

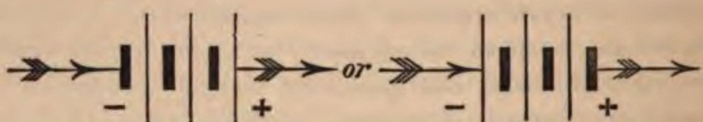
10. "Density of current" and "intensity of current" often cause great confusion. "Density of current" should only be used in the case of electrolysis or electro-deposition of metals. Here it means the ampères per unit of surface of the cathode. In electrostatics "surface density" or "electric density" means the quantity per unit area of surface. "Intensity" was used at one time in the same sense as "electro-motive force" is now, and therefore not so much out of place there; but several writers, notably Prof. Silvanus Thompson, have thought fit to borrow the French term "*intensité de courant*" wholesale, instead of a translation thereof, and to symbolise it by the letter I. The literal translation of the French word *intensité* being strength or amount, therefore the expression "current strength," or simply "current," symbolised by C, is far preferable, for it conveys the correct meaning of the quantity in a given time. With a little persuasion, French electricians would no doubt agree to the symbol C instead of I, to promote uniformity. Then I might be reserved for intensity of magnetism, where it suits very well.

11. "Positive electrode," "(+) terminal," "zincode," "anode," "positive pole," and "negative plate," severally used by different writers to designate that end of a cell, battery, or pile where the current leaves, and "negative electrode," "(−) terminal," "platinode," "kathode" or "cathode," "negative pole," "chlorous pole,"

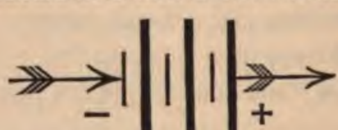
* See Ferguson's "Electricity," second edition, by Professor Blyth, p. 164. The term "electrokinematics" has been used by Clerk Maxwell to cover a large part of this subject, such as "electric current," "conduction," "resistance," "electro-motive force," "electrolysis," etc. (See "Electricity and Magnetism," by Clerk Maxwell, *Vol. I., Part 2.*)

"positive plate," where the current returns to or enters the same, requires revising and simplifying, more especially when we consider that the end plates of a battery are of opposite sign to their electrodes or terminals, and that the nomenclature is still further complicated when we come to consider secondary batteries or electrolysis by the terms "anion," "kation" or "cation," and "ions." Take, for example, the definition given by Sprague* of "anode": "The positive electrode or pole of a battery; the wire or plate connected to the copper or other negative element of the battery; the plate which leads the + current into a solution to be decomposed, and at which are set free the oxygen, acid radicals and all—ions (anions). In electro-metallurgy it is usually formed of the metal to be deposited, in which case it is called the soluble anode or pole"!!

12. Again, we have the two different ways of graphically representing a battery



according to the whim or fancy of the writer. Practical submarine electricians were the first to use this very neat and handy way of representing a battery and its poles, and always adopted the former method, with the long thin vertical line for the plate where the current leaves, and the thick short line for the plate where the current returns to the battery. Why should this have been departed from? It is a mere arbitrary arrangement, but, being a most convenient symbol, it should be used in a uniform manner. Mr. John Munro proposes that the symbol for a secondary battery should be a modification of this, viz.,



* Sprague's "Electricity," 1884 edition, p. 624.

† See Faraday's "Experimental Researches," articles 661 to 667, on *Definitions of New Terms,* where he very clearly points out the meaning he attaches to the words "electrode," "anode," "cathode," "ions," "anion," "cation," etc

the current outside the cell proceeding from the long line to the short one.

13. "Parallel circuit," "multiple arc," "loop circuit," "in loop," "derived circuit," "shunt circuit," are all expressions to signify pretty much the same thing, where one expression, "shunt circuit," would do.

14. "Polarisation" is a term used in many different senses—for example, the polarisation of battery plates, molecular polarisation due to electrification or magnetisation, polarisation of light due to magnetism, etc., as in Dr. Kerr's experiments. Some reform is required here.

15. Coming to telegraphy, telephony, and electric lighting, we find, as M. Hospitalier points out, "the words generator, receiver, transmitter, and motor are mixed up by different inventors, sometimes through ignorance, sometimes willingly." "A generator is an apparatus which, receiving energy of a certain nature, produces an energy of another nature, and it borrows its name from the nature of the energy which it generates. A receiver is an apparatus analagous to the generator, but it borrows its name from the energy which it receives." "A given apparatus is at once a generator and receiver—for example, an electric motor is a generator of mechanical energy and a receiver of electrical energy." "The name transformer ought to be reserved for an apparatus which, receiving an energy of a certain kind, reproduces or brings into play an energy of the same kind or of the same form." For example, induction coils as used on a trunk telephone line with several subscribers' lines at each end, or in electric lighting on the Gaulliard & Gibbs' system, lately tried in London. Professor Silvanus Thompson uses the phrase "Dynamo-electric Machinery" in the most general etymological sense of the term, as meaning machinery for converting the energy of mechanical motion into the energy of electric currents, or *vice versâ*, excepting such induction machines as Holtz, Voss, etc. He thinks this reduces the ambiguity to a minimum, and leaves the word "motor" to be applied, if desired, to the steam-engine, water-wheel, etc., from which the mechanical *motion* is derived. The terms "magneto-

electric machine," as applied to a dynamo fitted with permanent field-magnets, and "electro-magnetic machine" to a series, separately-excited, shunt, or compound-wound dynamo (generator or receiver), are very handy expressions, and should not be discarded.

16. We have dealt hitherto chiefly with definitions and nomenclature, and have given a few examples: others will occur to every member present. We now come to abbreviations and notation with symbols. The want of uniformity here, and the need for systematising, is still more obvious, but perhaps more difficult to accomplish. Every one admits the great advantage in being able to write down the symbols for chemical elements and their actions and reactions one with the other in the form of simple equations, which any one may comprehend who knows the subject, without a detailed description of what each letter or symbol stands for. Electricians should not rest satisfied until they are supplied with a similar universally-accepted notation, whereby electrical phenomena and actions may be similarly treated. The author submits a sample of what he considers would be useful in this respect. Many of them are taken from Munro and Jamieson's "Pocket-book of Electrical Formulæ," where an effort was made to use the same notation and abbreviations throughout, except in such cases as that of quoting direct from some other author.

It will be observed in this list that in most cases the first English or Greek letter of the word has been used. Those relating to the metric system have been copied from the French edition of Hospitalier's "Electrician's Pocket-book," which are no doubt copied from the list decided upon by the International Commission on the Mètre, with a few omissions and additions by the author.

The Greek letters π, μ, ϵ are universally adopted—π for the ratio of the circumference of a circle to its diameter, μ for the coefficient of friction, and ϵ for the base of Napierian logarithms.

Metric Abbreviations.

m. for mètre.
cm. „ centimètre.
mm. „ millimètre.

m^2.	for	mètre-square.
m^3.	„	mètre-cube.
c^2.	„	centimètre-square.
c^3.	„	centimètre-cube.
gm.	„	gramme.
mg.	„	milligramme.
kg.	„	kilogramme.
kgm.	„	kilogrammètre.
&c.		&c.
temp.	„	temperature.
res.	„	resistance.
g.d.	„	gramme-degré.
kg.d.	„	kilogramme-degré.

Electrical Abbreviations, Notation, and Symbols.

When a capital letter is used for the symbol, then small capitals or italics with suffixes, 1, 2, 3, etc., may be used for parts making up a whole. For example—L for length, L_1, L_2, L_3, etc., or l_1, l_2, l_3, for different lengths, or parts of L.

Fundamental and Derived Mechanical Units.

L	for Length.
M	„ Mass.
T	„ Time.
V or v	„ Velocity.
A or a	„ Acceleration.
F	„ Force.
δ	„ dyne; *e.g.*, $10\ \delta = 10$ dynes.
W	„ Work.
w	„ weight.
ft.lb.	„ foot pound.
H.P. or HP	„ Horse-power.
I.H.P.	„ Indicated Horse-power.
B.H.P.	„ Brake Horse-power.

Other Common Symbols allied to Mechanical Work.

D or d	for Diameter.
r	„ *radius.*

ω for angular velocity $= 2\,\pi\,n$ in radians per second.
g ,, acceleration due to gravity.
N or n_1 n_2 etc. ,, number of revolutions.
$-^{s}$,, second; *e.g.*, $3^{hr}\ 5^{m}\ 10^{s}$ = 3 hours 5 minutes 10 seconds.
$\tau_1\,\tau_2\,\tau_3$ etc. ,, temperatures, absolute.
or $t_1°\,t_2°\,t_3°$ etc. ,, ,, common.

Practical Electric Units.

The astronomical method of putting the small letters above the line of the figures, as in the case of the example $3^{hr}\ 5^{m}\ 10^{s}$ (3 hours 5 minutes 10 seconds), has not been followed in the following examples, as mathematicians object to the system, the letters appearing as if they were powers. Neither will they readily agree to suffixes, as suffixes have been already adopted by them to distinguish between things of the same kind. The author has therefore written the distinguishing letters on a level with the figures: for example, 10ω stands for 10 ohms (the methods 10^{ω} and 10_{ω} being both objectionable).

C.G.S. for centimètre, gramme, second.
R ,, Resistance.
ρ ,, specific resistance.
ω ,, 1 ohm; *e.g.*, 10ω = 10 ohms.
Ω ,, 1 megohm; *e.g.*, 10Ω = 10 megohms.
C ,, Current.
A ,, 1 ampère; *e.g.*, 10A = 10 ampères.
a ,, 1 milliampère; *e.g.*, $10a$ = 10 milliampères.
E ,, Electro-motive force or E.M.F.
v ,, 1 volt; *e.g.*, 10v = 10 volts.
K ,, Capacity.
σ ,, specific inductive capacity.
Φ ,, 1 farad; *e.g.*, 10Φ = 10 farads.
ϕ ,, 1 microfarad; *e.g.*, 10ϕ = 10 microfarads.
Q ,, Quantity (coulombs).
P ,, Power.
WP ,, Watts or watt-power.*
W ,, Work in joules.
H ,, Heat in ,,

* By adopting the term "watt-power," there can be no doubt what a *watt* means.

J for Joule's equivalent $= 42 \times 10^6$ ergs, or work spent on ·24 gm. of H_2O raised by 1° cent.

$C = \frac{E}{R}$ (Ohm's law).

$E \times C = C^2 R = \frac{E^2}{R} = \text{W}^{\text{P}}$ (watt-power).

$E C T = C^2 R T = \frac{E^2 T}{R} = E Q = W$ (joules).

$\frac{E C T}{J} = \frac{C R T}{J} = \frac{E^2 T}{J R} = \frac{E Q}{J} = \frac{W}{J}$

$= W \times \cdot 24 =$ g.d. or gramme degrees.

z = Electro-chemical equivalent.

Magnetism.

N for North pole of a magnet, painted blue.
S „ South „ „ painted red.
m „ magnet strength (of pole) or quantity of magnetism.
l „ distance between the poles of a magnet.
M or ml „ moment of a magnet.
$\mathfrak{J}$ or I „ Intensity of magnetisation.
μ „ magnetic permeability.
κ „ magnetic susceptibility.
H „ Horizontal intensity of terrestrial magnetism.
θ „ angle of deflection.
r „ radius (mean) of a coil or solenoid.
n „ number of anything; *e.g.*, turns of wire in a coil or galvanometer.

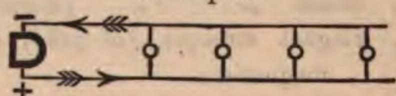
Take Tangent Galvanometer Formulæ, as an example to illustrate the above:

$$\text{Current, } C = H \frac{r}{2 \pi n} \cdot \tan \theta.$$

G	for	Galvanometer or galv. res.	$\therefore R = B + \dfrac{G\,s}{G+s} + r$ *
s	,,	shunt res. for galvanometer	
r	,,	resistance coils	
B	,,	Battery or battery res.	

Dynamo Circuits Contractions.

Example:



or D for Dynamo.
T+ ,, Terminal positive.
T− ,, Terminal negative.
E.M. ,, Electro-magnet.
or F.M. ,, Field Magnet.
c.p. ,, candle-power of a lamp.
A.M. ,, Ampère-meter.
V.M. ,, Volt-meter.
S.W.G. ,, Standard wire gauge.

For use in Formulæ.

R_a R_m R_e ,, Resistance of armature, magnets, and external circuit respectively.
C_a C_m C_e ,, Current in armature, magnets, and external circuit respectively.
L_s ,, coefficient of self-induction.
L_m ,, coefficient of mutual induction.

* See Munro and Jamieson's "Electrical Pocket-book," p. 65.

In the above notation the first letter of the most important words has been used wherever it was found practicable to do so, and the recurrence of the same letter under similar circumstances avoided as much as possible. In cases where no ambiguity can occur, such as H for the heat in joules, and H for the horizontal intensity of the terrestrial magnetism; *m* for mètre, and *m* for magnetic strength of pole; V for velocity, and v for volts, it will be observed that the same letters appear in each case.

The PRESIDENT: Mr. Jamieson has set us all a good example, and has proved himself a man of his word. He was kind enough to promise his paper last season, and to-night he has given it. It is on a very important subject, and one in which a great deal of interest at present is being taken. There can be no doubt whatever that if we could all speak in the same language, and write in the same signs, very great advantages would be derived from the system, and it would greatly facilitate and help the comprehension of students in our sciences. The Council to-night have been considering this matter, and they attach very much importance to it, and with a view to assisting in attaining such a desirable end they have decided to form a committee to inquire into this question. We wish to make this a very wide committee, because we want to get all the information we can from all the sources which are likely to prove useful to us, and therefore propose at first throwing it open not only to the members of this Society, but to professors of natural philosophy at the different colleges who take an interest in the subject, and who would be able to render us great assistance from their knowledge in scientific matters. We also propose to invite M. Blavier, the President of the International Society of Electricians, and also M. Hospitalier, who is the Secretary of that Society. He has shown so great an interest in this subject, and has done us the favour of coming here to-night from Paris to give us his views and take part in our discussion, that I am sure you will all give him a very hearty reception, and say we are very pleased to see him; and if we can get our committee on this side of the water to work with the gentlemen forming the committee on the other side, we ought then to be able to come to some definite conclusions that will be

in the future simple and satisfactory to all concerned. I will now ask M. Hospitalier if he will kindly give us his remarks on this paper.

M. E. Hospitalier: La question dont M. Jamieson vient de nous entretenir intéresse tout autant les hommes de science pure que les praticiens.

La Commission des *Notations électriques* nommée par la Société Internationale des Electriciens, présidée par M. E. Blavier, et au nom de laquelle j'ai l'honneur de vous entretenir ici à titre de secrétaire, s'est aussitôt mis à l'œuvre et a accompli une partie de sa tâche, celle relative aux abréviations, notations, et symboles. Elle abordera bientôt la seconde partie, celle qui a trait aux définitions et aux conventions.

Sans entrer dans le détail des décisions prises sur chaque point, dont vous trouverez le résumé dans les épreuves d'avant-projet que j'ai remises au bureau de votre Société, et que vous pourrez consulter. Je vous demande la permission d'esquisser la grande ligues de cet avant-projet.

Dans toutes les grandeurs physiques dont nous faisons usage on a à considérer :

1. La grandeur physique elle-même, en dehors des unités qui servent à la mesurer.

2. L'unité C.G.S. qui sert de mesure à cette grandeur: nous avous admis en principe que nous adoptions le système C.G.S.

3. L'unité ou les unités pratiques qui ont, en général, un nom spécial pour chaque espèce de grandeur et sont un multiple ou un sous-multiple *décimal** de l'unité C.G.S.

4. Enfin, des multiples et des sous-multiples décimaux de ces unités pratiques, multiples d'un usage courant.

Après discussion, la Commission des Notations a décidé d'adopter toujours une grande lettre, une majuscule, pour désigner la grandeur physique, une petite lettre pour l'unité C.G.S. lorqu'elle a un nom spécial; une petite lettre pour l'abréviation de chaque unité pratique, et enfin des préfixes

* Excepté pour le temps et les angles.

toujours les mêmes, pour les multiples et les sous-multiples décimaux des unités pratiques.

Ainsi, par exemple, le travail s'indiquera par la lettre W (du mot anglais *work*) l'unité C.G.S. sera l'erg que nous écrirons erg, sans abréviation, le mot étant très-court, les unités pratiques seront le kilogrammètre (kgm.), le grammètre (gm.) &c. Le multiples seront le meg-erg, la tonne-mètre (t.-m.), &c.

Pour la puissance, nous aurons la lettre A (du mot anglais *activity*), l'unité C.G.S. sera l'erg par seconde (erg : s.) les unités pratiques seront le kilogrammètre par seconde (kgm. : s.), le cheval-vapeur (ch.-v.), le watt ou volt-ampère (v.-a.), &c.

A ce sujet, nous vous demanderons de vouloir bien supprimer le mot *unit*, qui sera plus facilement compris en l'appelant *kilowatt* (k.v.a.) la quantité que l'unit représente. Une machine dite de 4 *units* sera une machine de 4 kilowatts, et tout le monde saura ce que cela veut dire, tandis que le mot *unit* n'a pas de sens dans le cas particulier.

En faisant dériver les abréviations des définitions, la Commission a cherché a demander le moins possible à mémoire et le plus possible au raisonnement. Cette méthode a l'avantage de permettre de créer des mots nouveaux, dans l'avenir, à mesure que le besoin s'en fera sentir dans la pratique, et cela presque automatiquement.

On a voulu aussi ne prendre dans les abréviations que des caractères faciles à trouver dans toutes les imprimeries, et c'est pour cela que les lettres grecques ont été systématiquement écartées presque partout.

C'est par ces caractères généraux que l'avant-projet de les Commission des Notations électriques diffère si notablement des propositions faites par M. Jamieson. Je n'insiste par davantage, puisque vous denez nommer une Commission chargée d'étudier la question, et que vous avez fait à la Société Internationale des Electriciens le grand honneur d'appeler le president et le secrétaire de la Commission des Notations électriques, à venir partager vos travaux. Nul donte que de notre entente commune, et des sentiments de bonne confraternité dont nous sommes tous animés, ne sorte bientôt une *œuvre utile* et féconde dont la science électrique *ne tardera par à recueillir* les fruits.

Professor Forbes: A very large subject has been broached by Mr. Jamieson, and M. Hospitalier has succeeded in making it even wider. I think that we cannot expect to go very far into the question this evening, because, really, if we were to say all that we felt inclined to say on the subject, it would be to anticipate the labours of the committee which it is proposed to appoint hereafter. I think the most one can do is to look over the proposals both of Mr. Jamieson and M. Hospitalier, and see whether in their general character they seem to commend themselves to us. Now, looking over Mr. Jamieson's paper, the first part of it deals with general terms—with words which are in constant use as applying to electrical subjects—and I think every one of us here must have experienced enormous confusion from the large number of terms which are used to mean the same thing, and the large number of things which are expressed by the same terms; and I am sure still more will this have been so with those who have had anything to do with the teaching of students, and those who have to look over the examination papers of students (and here I speak feelingly), who are painfully aware of the frightful confusion in some of our terminology. All these confusing terms which Mr. Jamieson has brought forward certainly deserve some examination, and I am sure it would be a great blessing to those who write, as well as to all of us who discuss electrical questions, if any authoritative body, such as the committee which it is proposed shall be appointed, should suggest anything which would be scientific and right; and I feel sure that everybody who had occasion to use these terms would be only too glad to avail themselves of those suggestions, and that a great deal of the confusion arising out of the use of different words for the same thing would disappear. All that is required, I am sure, is some authoritative suggestion as to which term, of two both equally permissible, should be adopted.

Mr. Jamieson suggests that the terms "vitreous" and "resinous" should be abolished, and I think that most of us will agree with him; but I do not agree with him in his reasons why we should drop the terms "positive" and "negative." He says that *these terms* "should be discarded for the modern theory of electric

polarity of molecules or continuous particles, expressed by positive (+) and negative (−)." I do not think we should introduce any theory whatever into our nomenclature. The less of it the better. Most of us are perfectly agreed as to the probability of such theories, but the less reference we have to theories the better. The terms "positive" and "negative" I would choose, because they assume no theory. They contain no reference of any sort, no notion of any theory whatever; they are simply opposite to each other.

Then as to "tension" and "potential" and "electro-motive force," there has been a great deal of confusion. Latterly I have very frequently—especially in speaking to popular audiences—felt it very satisfactory to use the word "pressure." I first used it after I found it had received Parliamentary sanction, and I suppose that really Mr. Moulton is responsible for this term. The scientific meaning I attach to the word "pressure" would be the difference of potential at the point where the pressure is indicated; that is to say, that pressure should only be used when you have two wires, when you are really meaning the difference of potential between two contiguous points of conductors. It is such an extremely convenient word in connection with electric lighting, and is comprehensible to those who are more accustomed to deal with hydraulic questions than with electricity, and it is very desirable to give it a scientific definition.

Then we come to the question of magnetism, which has always been a standing doubt. Mr. Jamieson says: "The pole which turns towards the geographical north is variously called the 'austral pole,' 'north pole,' 'north-seeking pole,' 'marked pole,' and is painted red by Sir William Thomson, while the late Sir George Airy, Professor Guthrie, and others, paint it blue." But he does not state that besides being called the "north pole" it is also distinctly called the "south pole." Further on he says that Sir William Thomson called it the "true south pole," but that ought to have been included among the various terms which are given. Now, Sir William Thomson's argument on this point is that it will be most confusing; you cannot put it into the head of a common *sailor—an uneducated* sailor—that the magnetism at

the north pole is a south pole, and that it is contradictory to the sailor's views of what is right. My answer is that you will never get the A.B. seaman to believe that the north end of the needle is a south pole. The only other argument which Sir William Thomson has ever used is that Gilbert used it—the founder of true investigation into magnetism. He used the term "north pole" for the magnetism at the north pole of the earth. However, these are all questions which will be discussed in the conference.

Mr. Jamieson says that "compass-error" ought to be used instead of "deviation." But compass-error is not the deviation. The compass-error, as the author justly remarks, is the angle which the meridian of the compass-needle makes with the true north and south magnet bearings. But the deviation which sailors speak of is independent of the variation and does not include the variation. The deviation is simply the part dependent on the ship's magnetism. I think that Mr. Jamieson is a little in error when he speaks about Sir William Thomson's compass as having no compass-error, and that only when there is "a change of cargo or in the position thereof (if of steel or iron), or due to buffeting the waves for some time on one course," will a slight error creep in. Now in some of the ships with whose compasses I have had most experience—the Pacific Steam Navigation Company's ships—when going into southern latitudes, Sir William Thomson's compass, or any other, would be in error, for you cannot distinguish by the ordinary methods between the permanent magnetism and the induced magnetism of vertical rods of iron.

I am glad that Mr. Jamieson has raised a protest against the general use of the word "polarisation." Polarisation requires a much more defined use than what it has received in the past. I remember a friend of mine going into an electrical factory, wanting to do an experiment, and asking whether they had a resistance-box, and the answer was (merely going on the notion that if anything is wrong with a battery it is polarised) that they had a set of resistance-boxes, but that they were polarised!

I will not attempt to go into the various letters which have been proposed by Mr. Jamieson. I think, on the whole, they are *extremely* suitable, and a very good foundation for the committee

to work upon. At the same time I quite agree with M. Hospitalier's criticism. I think there is a little inconsistency. I think in the words M. Hospitalier addressed to us every one must have been pleased with the attempts to gain consistency of nomenclature which the French Committee have made, and this, I think, is a very important thing indeed. There is one thing that in this large system one is struck with, and that is the enormous demand for alphabets; and I think it is perfectly wonderful what Mr. Jamieson has done with a limited number of alphabets; and when we think of the number of ways in which every unit is to be expressed which M. Hospitalier wrote on the board, viz., physical, theoretical, absolute, and practical—four different systems in which different terminals are used for each one—we shall soon use up the whole of the letters of the alphabet. For the C.G.S. units we have got words. We want symbols for them. For a great deal of electrical work we can arrive at one result in C.G.S. units most easily, and then convert them into practical units—volts, ampères, &c. I quite agree with M. Hospitalier that it is better not to use K for "capacity." I think I should almost be inclined to give them the I for "current." The Germans use the I (and, by the bye, that is a point: we should try to get German co-operation in this matter as well as English and French), and K, as M. Hospitalier has justly remarked, stands much more appropriately for "constant" than for anything else; at least we have always been in the habit of using it so. I also agree with M. Hospitalier in disapproving of using the word "unit" in the way the Board of Trade use it—to mean simply a thousand watts for one hour. Still more do I dislike the way in which we find Mr. Crompton making dynamos—of so many units. If a dynamo of six units is capable of only giving six units, it is a very short-lived dynamo; its life is only one hour. Of course we all know what he means. But I think "six thousand watts" would be better than introducing the Board of Trade term, which is a doubtful thing. I am sure that every one of us will feel that we owe a great deal to the French Committee for the remarkable concession which they have made to us after the long time we have striven to *get the poor W into* circulation. I am very sorry that

M. Hospitalier (most justly, I must allow) objects to the two symbols HP and WP. They would be very convenient. Mr. Jamieson has said that we should not have confusion of terms amongst such men as Mr. Preece and Professor Adams. Well, if such a symbol as this were used for horse-power, HP, and watt-power, WP, perhaps I might suggest that this one might be further developed to prevent still more confusion, combining the two in this way, WHP, for watt-horse-power, as being the initial letters of one of those gentlemen's names. (Laughter.)

Mr. J. Munro: With regard to the proposed abbreviations, I think there ought to be some rule by which we could know them, without having to learn them like a code of signals. If the first letter of the principal syllables were taken to make a contraction for the word it might be an advantage. Then there is the question of graphic symbols, which has not been touched upon to-night at all. We require certain graphic symbols in drawings, and I hardly think there are sufficient of them now. Up to the present those we have had were in general sufficient for telegraph-engineers, but I think a few more are wanted—not very many—for dynamos and secondary batteries, for instance, and to distinguish arc from incandescent electric lamps. We might also add one or two for telephonic instruments. These graphic symbols can be recognised at a glance, and do not require any letters, so that an engineer, in drawing a sketch of an installation for his assistant, can do it without requiring to write explanations on the drawing. Moreover, if they were adopted universally by writers, an electrician who did not know a foreign language could trace out the connections of an installation from the plan. I think the use of these graphic symbols might well come under the consideration of the committee.

Professor Ayrton: The importance of the subject has been so fully entered into by the author of the paper, and by M. Hospitalier and Professor Forbes, that it is not necessary for me to dwell at all on that point. I quite agree with Professor Forbes that it is undesirable that the names should in any way suggest certain theories, because, if a theory is found to be *wrong, then it* would be a sort of reason for changing the name.

It is undesirable to adduce any kind of theory in assigning a name, and especially to adduce a wrong theory. Under 4, on page 2 of the slip, Mr. Jamieson has said: "The term 'accumulator' is the name given in several text-books to apparatus, such as the Leyden jar or condenser, for receiving and retaining quantities of electricity, but has been lately inappropriately applied to secondary batteries, which do not accumulate electricity." I quite agree with the last part of the sentence—"which do not accumulate electricity," but I presume from the context that Mr. Jamieson would say that a Leyden jar does accumulate electricity. Of course that is utterly wrong in principle. There is absolutely no difference externally in the action of the Leyden jar and a secondary battery, which can be shown in a moment, and therefore if the term "accumulator" is applicable to one it is applicable to the other.

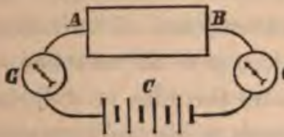


FIG. 1.

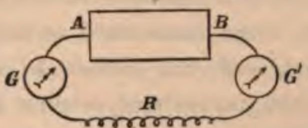


FIG. 2.

Let A B, Fig. 1, be the terminals of a box of such a nature that, when the terminals of a generator of electric current, C, are attached to A and B, and two galvanometers, G and G′, are put in circuit, it is found that G and G′ indicate always the same current, the one as the other, but a current that grows less with time. Further, let the box and its interior be of such a nature that when the generator, C, is removed, and the wires joined together through a suitable resistance, R, the currents flowing through G and G′ are equal in strength but opposite in direction to the previous charging currents, and diminish in strength with time. Then how is it possible to decide whether the box contains a secondary battery of small storage capacity or a Leyden jar with a very large capacity? Both in charging and in discharging as much electricity passes out as passes into the box, and this is exactly what would happen whether the box contained a secondary battery or a Leyden jar. *In neither a secondary battery nor in a*

Leyden jar is there the slightest storage of electricity, and in both there is a simple storage of electric energy. Neither is an accumulator of *electricity; both* are accumulators of *electric energy*.

I regret that I cannot agree with Mr. Jamieson in wishing to confine the word "potential" to electrostatic actions, and to use "electro-motive force" for "electrokinematic." The fact is the word "potential" or "difference of potential" cannot be discarded: it contains a totally different idea from electro-motive force. For example, you speak of the electro-motive force of a battery or dynamo machine, and you speak of the difference of potentials between the terminals of a battery or of the dynamo machine, which latter is what a voltmeter measures, and you have a perfectly clear conception of the difference between the two. You must keep the idea of difference of potentials between two points of the circuit through which a current is passing. If you like to adopt some word as an abbreviation of "difference of potential,"—if you choose to seek some abbreviation,—very good; but you certainly cannot discard the idea of difference of potential between two points of a circuit in electrokinematics.

I would suggest that "difference of potentials" should be shortened into "potential difference," and that this should be simply indicated by P.D., just as "electro-motive force" is represented now very frequently by E.M.F.

With reference to painting the ends of a magnet, except for decorative purposes, I must agree with M. Hospitalier. I do not see what can be gained by the plan, and I certainly never did know whether the red end pointed to the north or to the south. No doubt there is a certain difficulty with students. Like Professor Forbes, when I was a student of Sir William Thomson's I was taught to call the pole that points to the north the "true south" pole, and not the "north" pole, and for a long while after leaving Glasgow I did call it a "true south" pole, and I did not like to call it the "north" pole, although tempted to do so. Then, still with the fear of Sir William Thomson in my eyes, I called it the "marked" pole, because in France, as in England, that end of a magnet has frequently a mark put on it. Then I got into the

habit of calling it the "north-seeking" pole. Now I am afraid I often call it the "north" pole!

I must confess I do not follow Mr. Jamieson in his idea that the improvement Sir William Thomson has effected in his compass does away with the names we shall give to the errors of the compass. I know that his compass is very much better than others, but I do not see that because it has overcome certain errors it has done away with the necessity of names for those errors. Mr. Crompton has done a good deal to overcome the sparking of dynamo machines, but I do not see how that decides whether we shall call the fault in other dynamos "sparking" or "flashing" at the commutator. Words must still exist to express the various errors of the compass, otherwise we cannot refer to the improvements that have been effected.

Then, again, the expression "shunt circuit" would hardly express all cases. For example, you can join up cells in parallel or in series; and I do not think you could conveniently say, when you had cells joined up in parallel, that they were joined up in "shunt circuit." I think you must keep to "in parallel." It is a good expression, and conveys a very great deal of meaning.

Now with regard to the name for a dynamo-electric machine. The word "dynamo," dropping the rest of the name, was first, I believe, suggested by Sir William Thomson; and the word "motor," to stand for a machine that converts electric into mechanical energy, my colleague and myself are, I believe, responsible for. It appears to me that they are very expressive, and I cannot imagine that anything would be gained by discarding them. There is no fear of confusing the name "motor" with that for a steam-engine. I would rather suggest that the three terms, "engine," "dynamo," and "motor," should be more generally adopted for the machines respectively for converting heat energy into mechanical energy, mechanical energy into electric energy, and electric energy into mechanical energy.

Regarding abbreviated symbols as distinct from abbreviated names, there is a system of symbols that I have used in my lectures for several years, and found most convenient; and that is, to use *capital letters for currents* and small letters for resistances.

At the end of a long investigation, or, indeed, at any step during that investigation, it is often extremely convenient to be able to see at a glance how the currents and how the resistances enter. In starting, for example, with some calculation about a shunt dynamo, I would call A, F, and R the currents in the armature, field magnets, and outside resistance, and a, f, and r the resistances of these three parts respectively; so that the fundamental equations, if E was the electro-motive force of the machine, would be

$$A\,a + F\,f = E;$$
$$F\,f = R\,r.$$

I do not see why we should, with all due deference to the opinion expressed by Mr. Jamieson, add the word "power" to "watt," any more than we add the word "time" to "hours." When you speak of coming in "three hours," you do not always say "three hours' time." To talk of a "horse-power" and a "watt-power" makes it appear as if a watt was a kind of pony.

There is one other point that I will allude to, and that refers to the contractions that have been described by M. Hospitalier. Instead of using the expressions he has suggested, I would like to ask whether 10 with a 6 over it is not the very best contraction for a million? It has, at any rate, this advantage, that it is already universally adopted, and I much doubt a better contraction being suggested.

Mr. Biggs: I should like to say one word to show the difference of opinion between a good many people on this subject. I object to what Professor Forbes said as to using I for "current" and K for "capacity." I should prefer to use C for "current" and Cp for "capacity"—a system first introduced into electrical notation by Kempe in his text-book. I think this is following the plan that is adopted by chemists using initial letters as the symbol for the important element, and initial and a small letter for the symbol of the element of less importance. You have "current," which is more important, and "capacity," which is less important: use C for "current," Cp for "capacity." That would do away with K altogether.

Dr. Fleming: A few years ago there arose a discussion in the

pages of the *Electrician* on the use of the word "potential," and after numerous letters had been written on the subject the late Professor Clerk Maxwell wrote a very characteristic letter, in the opening sentence of which he said: "There is no more satisfactory evidence of the progress of a science than when its cultivators, having settled all their differences about the connection of the phenomena, proceed to reconstruct its definitions. Even in the most mature sciences, such as geometry and dynamics, the study of definitions still leads original thinkers into new regions of investigation; and in a science like that of electricity, the growth of which has proceeded from so many different centres, it is only to be expected that when the different departments have grown till they meet, their definitions will need some adjustment in order to make then consistent with each other." If the writer of these words could have been here to-night, I am sure he would have taken part in the discussion with very great interest, on account of the great importance he attached to exact definitions. In the above letter he attacked the vague use of certain words, such as "electric tension," used in an inexact way to express the tendency of electricity to escape from a body without any very clear notion of where it wanted to go. Professor Maxwell was a great opponent of terms which involved any hypothesis. He preferred to speak of the "quantity of a body's electrification" rather than of the "quantity of electricity in it," as involving less hypothesis; and, in particular, he used to observe that the use of the word "polarisation" by many writers required examination in order to see if they knew what they meant by it. It would take up too much of your time, at the present late hour, to go into the details of Professor Jamieson's valuable paper. In fact, we should be beginning the work which will occupy the attention of the committee. I would venture, however, to make one suggestion, and that is, that in the codification of symbols of quantities attention should be paid to the symbols adopted in Clerk Maxwell's great treatise, which must always be regarded as the *principia* of electricity. These symbols were selected by him with great care and judgment, and their uniform use in formulæ would be a *great advantage*. One thing I do hope will be settled

is the nomenclature of the plates and poles of primary and secondary batteries, on which there is at present very great confusion, and which it is exceedingly desirable should be cleared up.

Mr. J. Brailsford Bright: I cannot see why in some cases the letters should not be simply inverted, and thus avoid the necessity of borrowing from the Greek or other alphabets. Thus, for instance, the distinction between the symbol used for "current" and that for "capacity" might be very well obtained by choosing the C in one case and inverting it in the other. I do not see any difficulty in such a plan, which might be rendered both simple for the printer and free from inconsistencies.

Mr. Shoolbred: It seems hardly necessary to point out the value of Mr. Jamieson's communication, as it leads up to the correction of a want which has made itself felt so often to us all. I myself had occasion, in a paper "On the Measurement of Electricity," communicated to the Society two years ago, to deal with the practical units, and also the C.G.S. units, that have been referred to this evening. I then experienced how much confusion there existed in the minds of many, not merely as to those units, but also in respect to other electric terms and expressions. We owe a debt of gratitude to Mr. Jamieson, and also to the committee now proposed by the Council, whose valuable labours will be directed towards the settlement of these questions. To the long list of terms requiring clearer definition many others might be added. "Bobbin" I would suggest, since it is made to apply by different makers to quite different parts of a dynamo; and considerable confusion arises therefrom. In telephones uncertainty arises from the convertibility of the terms "transmitter" and "receiver."

With regard to M. Hospitalier's objection to the Board of Trade unit, as it is termed, of "1,000 volt-ampères," on account of its largeness, and in which objection Professor George Forbes coincides, I fail to see the inconvenience, especially if the word "kilowatt" be substituted, as was suggested some time back by Mr. Preece. It is not the ultimate unit of electrical measurement which it is intended to provide by it, but a convenient commercial *quantity to* reckon by. No objection on the score of magnitude is

made against one ton being the basis of quantity in the sale of coal, nor to many other quantities which may be termed units of measurement, each in its particular line. Indeed, some of the electric "practical" units are themselves open to objection on the score of magnitude—take the "farad."

Sir William Thomson, in his lecture before the Institution of Civil Engineers, May 3rd, 1883, "On Electrical Units of Measurement," spoke of 15·3 French tons being the universal-gravitation unit of matter. Surely a "kilowatt" would be a small matter after the above.

It is gratifying to find, from the draft report of the French Committee of the International Society of Electricians, how much has already been done by them towards remedying the evils pointed out by Mr. Jamieson. It is therefore to be hoped that the joint labours of that committee, and of the one about to be appointed by this Society, will diminish the confusion in electrical nomenclature which certainly exists.

Mr. H. E. Harrison: I must really raise a protest against the box Professor Ayrton drew on the black board. If I followed him correctly, he stated that if he had a condenser, used in the Leyden jar sense, or a secondary battery, placed in his box, he could not distinguish the difference between the two, because they are both capable of producing the same external effects, and that therefore any distinctive name or symbol applied to either of them would involve theoretical considerations which would be entirely foreign to the spirit in which the whole subject should be approached. Now I venture to express the opinion that if I were to give Professor Ayrton a condenser, or a secondary cell, shut up in a box, he would be able to tell me which it contained without looking inside. Assuming, however, for the sake of argument, that he could not, then, on the same principle, he ought not to distinguish between dynamo machines, thermopiles, batteries, or any arrangement producing a current, because, put any one of them into a box where you cannot see it, and it becomes simply a source of current. The system adopted by Professor Ayrton of using a capital letter for a current, and the corresponding small letter for the *resistance* through which it flows, is one which I

have myself frequently adopted in my classes; but I find this objection to it, and doubtless he has found it too, that the alphabet becomes exhausted too soon, and there is nothing left with which to express any other quantities. The use of suffixes undoubtedly has the objections Dr. Fleming has pointed out, but I think of the two it is the lesser evil. Writing on ruled paper, it is very easy to put the suffixes just below the line; and as to difficulties with the printer, you have only to worry him enough to get them right. The advantages of being able by means of suffixes to multiply the alphabet to any extent, seems to me to far more than compensate for the few disadvantages the notation may possess.

Professor AYRTON: Mr. Harrison has misunderstood me. I did not say that you should not have distinct names to distinguish a secondary battery from a Leyden jar; but what I did say was that you ought not to take a name that expresses a property common to the two, and apply that name to one of them, and say it will not apply to the other, or rather—what is still worse—take a name that expresses no property common to either, and say that the name is proper for the one and not for the other. It is quite right to say that both a secondary battery and a Leyden jar are "accumulators," if you mean accumulators of electric energy; but it is equally wrong to call either of them accumulators, if you mean accumulators of electricity.

Mr. SWINBURNE: It might be advisable for the committee not only to discuss mere questions of notation and terminology, but also to settle definitely what some of the units shall be. I have lately* called attention to the dreadful confusion we are getting into through having three sets of ohms and volts in use. Whether the committee is to deal with such questions or not, I would respectfully suggest that the Council does not appear to be forming a representative committee, which is the only committee whose recommendations could ultimately have much weight. A committee which discusses questions chiefly concerning telegraph and electric light engineers should consist chiefly of telegraph and

---

* *Electrician*, March 7, 1885.

electric light engineers. But the intention seems to be to form it mainly of the various professors of physical science at different colleges. No doubt they may have very clear views on these points, but they are not really much concerned with them, and electrical engineers may have quite as clear views on the scientific sides of the questions. It is probably chiefly because the Paris Congress was not representative that the latest ohm has been adopted just enough to create confusion, and no more. To introduce the ampère in 1881 was easy; but to attempt to change the ohm and volt now is quite another thing. Perhaps if the Congress had been more largely made up of those commercially engaged in electrical work the confusion of ohms would not have been caused, and the Congress would not have weakened its influence by bringing forward such a standard of light in the face of such a large industry as gas lighting. Mechanical engineers have great respect for such men as, say, for the sake of alliteration, Reulaux and Routh; but a committee or congress of such men would have very little weight if it tried to introduce a decimal system of measurement, or to alter the wire gauges or standard threads.

Many electricians seem either to have very loose ideas as to the meaning of the term "current," or to use the word very carelessly. It is probably due to this that so many have come to confuse power and energy so curiously. The fluid analogy is universally adopted now, and we may compare electricity in motion to water, except, of course, in discussing such questions as capacity, when, as I have already pointed out,* a compressible fluid must be used. Adopting the analogy we must say that electricity flows, and current is the rate at which it flows. But from the expressions commonly used it would appear that many confuse "current" with "electricity," and think it is something that passes, and talk of "passing a quantity of current," and so on; whereas it is not even strictly correct to talk of "passing a current" at all, for it is the electricity that passes, and the "current" is the rate of flow. No one talks of "passing a power-strength of 60 watts

* "*Practical Electric Units.*" (Spon.)

through " a lamp; and such expressions as "current strength," "*intensité de courant*," &c., are about as apt as "intensity of income tax."

Mr. Walmsley: I do not rise to-night on my own behalf, but for Professor Silvanus Thompson, who, being unable to come himself, entrusted me with a communication to the Secretary. On presenting it I was informed that the contents ought to be read by a member of the Society, and therefore, with your permission, I will read what Dr. Thompson asked me to hand to the Secretary:—

As the author of the paper has mentioned me by name under some misapprehension, I think, of the matter to which he refers, I beg leave to offer the following comments. Mr. Jamieson seems to think that because I have, both in my "Elementary Lessons in Electricity" and in my later work on dynamo-electric machinery, used the symbol I for expressing the strength of the current instead of the letter S, or the letter C, or the letter A—any one of which might be justified by authority—that therefore I propose "to borrow the French term, *intensité de courant*, wholesale, instead of a translation thereof." I do not propose to borrow the obsolete term, even though the translation thereof—"intensity of current"—does happen to begin with an i. I have used, and intend to continue to use, the letter i for number of ampères of current, because I find that it gives rise to fewer ambiguities in notation than either of the letters C, for current, and A, for ampères, and is already more extensively in use for that purpose than either C or A. I do not, by the way, understand why, if Mr. Jamieson has already placed A for ampères in his list, he needs also to write C for current. Does he intend to use some other unit of current than the ampère? If not, why use C for current as well as A for ampères?

As I believe great gain will accrue to all electricians by a consensus as to the notation best for universal use, and as it is very inadvisable that any narrow insularity should prevent us from agreeing with our Continental brethren on so simple a matter, I am the more desirous of explaining why I propose and *intend to* continue to use i rather than C or A for expressing the

number of ampères of current. The letter C has already to do duty in our formulæ as a symbol for any arbitrary constant—such as constants of integration—and it also is used for capacity. It is most unwise to use the same symbol for capacity and for current. Mr. Jamieson proposes K for capacity, but K is already universally adopted for the coefficient of specific capacity (or dielectric constant). It may, too, be remarked that σ, which Mr. Jamieson gives as a substitute for specific inductive capacity, is already used for surface-density of charge. As we cannot use K both for the whole capacity of a condenser and for the specific capacity of the dielectric in it, it is better to retain K in its present use, keeping C for the capacity of the condenser, for which it is doubly appropriate.

In illustration of the widely extended use of i as a symbol for current-strength, and in illustrating also the curious differences of practice which exist amongst electrical writers, I here give all the various ways used for expressing Ohm's law so far as I have been able to find them. I begin the list with the great discoverer of that famous law.

Symbols adopted by various Authorities to express Ohm's Law.

Ohm	$S = \frac{A}{L}$	("Die Galvanische Kette," p. 36.)
Thomson	$\gamma = \frac{F}{k}$	("Math. and Phys. Papers," p. 497.)
Wheatstone	$F = \frac{E}{R}$	("Memoirs," p. 99.)
Wiedemann	$I = \frac{E}{W + w}$ and	
"	$I = \frac{E}{R + W}$	
Jenkin	$C = \frac{I}{R}$	("Electricity," p. 82.)
Mascart	$M = \frac{V_1 - V_2}{\lambda}$	("Traité d'Électricité Statique," Vol. II., p. 451.)
"	$M = \frac{E}{\lambda}$	(*Ib.*, p. 501.)

Mascart & Joubert ...	$I = \frac{E}{R}$ ("Leçons," p. 271.)
" " ...	$I = \frac{V}{R}$ (*Ib.*, p. 272.)
Culley	$C = \frac{E}{R}$
Latimer Clark	$I = \frac{E}{R}$
Maxwell	$E = C R$ ("Electricity and Magnetism," art. 274.)
"	$E = I R$ (*Ib.*, art. 345.)
Everett	$C = \frac{E}{R}$
Guthrie	$Q = \frac{E}{R}$
Sprague, J. T.	$C = \frac{E}{R}$
Carey-Foster (Lardner)...	$I = \frac{E}{R}$
Glazebrook	$C = \frac{E}{R}$
"	$V_1 - V_2 = R\, i$
Noad	$F = \frac{E}{R}$ (p. 301.)
Wormell	$C = \frac{E}{R}$
Siemens & Hopkinson ...	$C = \frac{E - \epsilon}{R}$
Chrystal	$i = \frac{E}{R}$
Ayrton & Perry	$C = \frac{E}{R}$
Lodge	$E = R\gamma$
Verdet	$I = \frac{E}{R}$
Ganot (Atkinson) ...	$C = \frac{E}{R}$
Deschanel (Everett) ...	$C = \frac{E}{R}$

ain & Bouty ... ... $i = \frac{E}{R}$

four Stewart ... ... $i = \frac{E}{R + r}$

guson ... ... $E = C\,R$

... ... $I = \frac{E}{R}$

aming ... ... $I = \frac{E}{R + r}$

don ... ... $C = \frac{E}{R}$

y ... ... $C = \frac{V}{R}$

(Gray is driven to use Cp for capacity, pp. 115 and 185.)

... ... $C = \frac{E}{R}$

Moncel ... ... $I = \frac{E}{R}$

ape ... ... $C = \frac{E}{R + r + G}$

iell ... ... $I = \frac{E}{R}$

... ... $J = \frac{E}{W}$

pitalier ... ... $I = \frac{E}{R}$

wbridge ... ... $S = \frac{E}{R}$

ler-Pouillet ... ... $J = \frac{E}{R}$

" ... ... $J = \frac{E}{W}$

ich ... ... $J = \frac{E}{W}$

ellen ... ... $J = \frac{E}{W}$

lner ... ... $e = \frac{E}{w}$

$I = \frac{E}{}$

Mousson	...	...	$I = \frac{E}{W}$
Heaviside	...	...	$E = R\,C$
Riemann	...	...	$J = \frac{E}{W}$

It will be seen by an examination of the foregoing list that there is a clear consensus so far as relates to employing E for the number of volts of electro-motive force, and R for the number of ohms of resistance. But opinion appears to be divided as between the use of I or C for the number of ampères of current, there being a clear, but small, majority in favour of using the symbol I.

Professor Hughes: At this late hour time does not allow me to discuss the several important points brought forward in the paper of Mr. Jamieson. In every paragraph there is much that requires mature thought, and as the whole subject is to be fully discussed by a representative committee, I have no doubt that we shall eventually adopt simple and clear definitions, avoiding as far as possible the adoption or expression of any theory. In this sense I have heard with great pleasure that both Mr. Jamieson and Professor Hospitalier propose to call the pole of a magnet which turns towards the geographical north the "north" pole; and this is sanctioned by almost universal recognition. For here we have an undeniable fact, dependent only upon the directive tendency of the poles, independent of any theory as to its cause, or reference to that of the earth's magnetism. In my numerous papers upon magnetism I have invariably followed this custom, preferring to call it simply the "north pole," and rejecting such confused definitions as the "north-seeking pole," "true south pole," "north-pointing pole," "marked pole," or "pole austral."

As regards the use of colours for indicating the poles, we know that it has been invariably the practice for makers to temper or colour the north pole blue, leaving the south pole bright. Lecturers, as a rule, adopt this colour for their apparatus, and, when necessary, indicating the south by red. There can be no *reason for* changing this, consequently I believe there must be a

slight error in Mr. Jamieson's paper, when he proposes to colour the north pole red instead of blue.

The whole question, however, of nomenclature and definitions will be thoroughly discussed by the committee, and I trust that by our labours, as well as those of the committee in France, we shall finally adopt terms clear in their definition, and which will be in a great measure international.

Prof. JAMIESON, having been invited by the President to make his reply, said: The time is so very late that all I shall say is that I am very glad to hear from the President that the Society intend to form a committee, and to meet the French Committee in the best possible spirit, and I hope that the outcome will be satisfactory and complete.

The PRESIDENT moved a hearty vote of thanks to Prof. Jamieson for his valuable paper, which was cordially responded to.

The PRESIDENT announced that M. Hospitalier had been kind enough to present the Society with the last edition of his new book as a contribution to the Society's Library. The thanks of the meeting were accorded to M. Hospitalier.

A ballot then took place, at which the following were elected:—

Foreign Members:

Charles Mourlon.
George West.

Associates:

Hector Dallas.
Alfred Ernest Mills, B.A.
James Oldham.
Corporal Albert Pike, R.E.
George Schultz.
Ronald Scott.
Alfred Watkins.

Students:

George Keith Buller Elphinstone.
E. Alexander Scott.

The meeting was then adjourned until Thursday, the 28th May, 1885.

THE LIBRARY.

ACCESSIONS TO THE LIBRARY FROM MARCH 25 TO MAY 1, 1885.

By Alfred J. Frost, *Librarian.*

(*Works marked thus* (*) *have been purchased.*)

IT IS PARTICULARLY REQUESTED THAT MEMBERS WILL PRESENT COPIES OF THEIR WORKS TO THE LIBRARY AS SOON AS POSSIBLE AFTER PUBLICATION.

* **Allard and others.** Expériences faites à l'Exposition d'Électricité. Méthodes d'observations—machines et lampes à courant continu, à courants alternatifs — bougies électriques — lampes à incandescence — accumulateurs—transport électrique du travail—machines diverses. 8vo. 152 pp. *Paris*, 1883

Amsterdam Exhibition. [*Vide* Mourlon.]

* **Ango** [A.] Les Enregistreurs Météorologiques. 8vo. 11 pp. ["Revue de l'Exposition Internationale d'Electricité." Groupe IV., Classe 12.] *Paris*, 1882

* **Anon.** Manuel de Télégraphie Electrique. 12mo. 72 pp. *Paris*, 1875

* **Anon.** Télégraphie Domestique. Instruction sur la pose et l'entretien des sonnettes. 8vo. 64 pp. *Paris*, 1865

Audra [R.] [*Vide* Nansouty et Audra.]

* **Bede** [E.] La Téléphone. Histoire description et applications des téléphones. La. 8vo. 113 pp. *Bruxelles et Paris*, 1881

Benét [Brig.-Gen. S. V.], Chief of Ordnance, U.S.A. Report to the Secretary of War for the Fiscal Year ended June 30, 1884. 8vo. 599 pp. Numerous plates. *Washington*, 1884
[Presented by the Ordnance Department, U.S.A.]

Berry [Graham]. Report upon the affairs of the Post Office and Telegraph Department of Victoria for 1882. Fo. 70 pp. *Melbourne*, 1883

* **Bjerknes** [M.] Phénomènes hydrodynamiques inversement analogues à ceux de l'électricité et du magnétisme. 8vo. 30 pp. *Paris*, 1882

Blanc. [*Vide* Le Blanc.]

* **Blavier** [E. E.] Considérations sur le service Télégraphique et sur la fusion des Administrations des Postes et des Télégraphes. 8vo. 126 pp. *Nancy*, 1872

* **Bonel** [A.] L'Electricité à l'Exposition de Bordeaux, 1882. La. 8vo. 59 pp. *Bordeaux*, 1882

Bordeaux Exhibition. [*Vide* Bonel.]

* **Boulard** [J.] Revue de l'Exposition Int. d'Electricité. Production et applications de l'électricité.
I. Machines Magnéto et Dynamo-électriques. Groupe I., Classe 3.
II. Moteurs Électriques et Transport des Forces. Groupe IV., Classe 9. La. 8vo. 156 pp. *Paris*, 1882

Bouty. Théorie des Phénoménes Électriques. 8vo. 84 pp. Plate. *Paris*, 1878

Brasher. [*Vide* International Telegraph Conference.]

* **Breguet** [Antoine]. La Machine de Gramme, sa théorie et sa description. Deuxième Tirage. 8vo. 78 pp. *Paris*, 1884

Cambridge Phil. Socy. Proceedings. Part III., Vol. V. Michaelmas Term, 1884. 8vo. 51 pp. *Cambridge*, 1885

* **Camille-Grollet** [Dr.] L'Electricite ses applications pratiques. 8vo. 340 pp. *Paris*, 1883

Cardarelli [F.] Lezioni di Meccanica Elementare dettate agli allievi Meccanici dell' Amministrazione dei Telegrafi. 8vo. 83 pp. *Roma*, 1885

* **Cazin** [A.] L'Etincelle Electrique. 8vo. 323 pp. *Paris*, 1880

* ——— Les Forces Physiques. 8vo. 322 pp. *Paris*, 1881

Clark [Latimer], **Muirhead, & Co. Ld.** Illustrated Catalogue of Telegraphic Apparatus, and Material used in the Construction and Maintenance of Electric Telegraphs, Electric Light, &c. With useful Tables and Formulæ. 8vo. 80 pp. *London*, 1885

Collette [J. M.] Le Télégraphe Imprimeur Hughes. Manuel principalement à l'usage des employés du Télégraphe. 8vo. 82 pp. Plates. *La Haye*, 1871

Conference. [*Vide* International Telegraph Convention.]

Convention. [*Vide* International Telegraph Convention.]

* **Cossmann** [M.] Applications de l'Électricité au matériel des chemins de fer. (Groupe II., Classe 6.) ["Revue de l'Exposition Int. d'Electricité."] La. 8vo. 117 pp. Plates. *Paris*, 1882

* **Darwin** [Erasmus]. The Botanic Garden. A Poem in 2 parts. 4to. 212 + 126 pp. [With Philosophical Notes.] *London*, 1791

D'Auge. [*Vide* Pelletier et D'Auge.]

De Nansouty [Max]. [*Vide* Nansouty, Max de.]

* **Desguin** [Pierre]. La Lampe Soleil étude raisonnée de ce système d'éclairage électrique. Conditions économiques de son emploi. La. 8vo. 39 pp. Plates. *Bruxelles*, 1881

* **Dumas** [Floridor.] Traité de télégraphie électrique militaire à l'usage des officiers de toutes armes et des fonctionnaires de l'administration des lignes télégraphiques qui peuvent être appelés à faire partie du service télégraphique civil en campagne. 8vo. 178 pp. *Paris*, 1869

* **Du Moncel** [Th.] Recherches sur la non-homogénéité de l'étincelle d'induction. 8vo. 115 pp. *Paris*, 1860

* ——— L'Eclairage Electrique. I. Generateurs de Lumière. II. Appareils de Lumière. 3rd Edit. 2 vols. 8vo. *Paris*, 1883

* **Du Moncel** [Comte Th.] **et Geraldy** [F.] L'Electricité comme force motrice. 8vo. 313 pp. 1884. *Paris*, 1884

* **Dumont** [Georges]. La vapeur et l'électricité appliquées aux arts et à l'industrie. 8vo. 238 pp. *Paris*, 1877

* **Duter** [E.]. Cours d'Electricité. 8vo. 265 pp. *Paris*, 1882

* **Estienne.** Etude sur les transmissions télégraphiques. 8vo. 44 pp. *Paris*, 1881

Exposition Internationale d'Electricité [1881]. [*Vide* Boulard].

——— [*Vide* Cossmann].

——— [*Vide* Picou.]

——— [*Vide* Nansouty et Audra.]

——— [*Vide* Fichet.]

——— [*Vide Ango.]*

* **Fichet** [L.] Les Téléphones. 8vo. 9 pp. ["Revue de l'Exposition Internationale d'Electricité." Groupe IV., Classe 7.] *Paris*, 1882

* **Figuier** [Louis.] L'Art de l'Eclairage. 8vo. 284 pp. *Paris*, 1882

* **Foucaut** [Dr. A.] Note sur l'isolement des conducteurs électriques. La. 8vo. 10 pp. [Ext. de la *Revue Maritime et Coloniale.*] *Paris*, 1875

Fournier. [*Vide* Urbanitsky.]

* **France** [Ministère de la Guerre]. Règlement Général du 19 Nov., 1874, sur la Télégraphie Militaire. 8vo. 42 pp. [Ext. du *Il Militaire Officiel*, partie réglementaire No. 84 de 1874, et du No. 30 de 1877.] *Paris*, 1877

Geraldy. [*Vide* Du Moncel et Geraldy.]

* **Giffard** [Pierre]. La Lumière Électrique expliqueé à tout le Monde. 18mo. 168 pp. With an Appendics by Richard Werdermann. *Paris*, 1882

Gisborne [F.] Telegraphic Communication with India. 8vo. 20 pp. [*Cambridge Essays*, 1857, pp. 105–125.] *London*, 1857
[Presented by W. T. Ansell, Esq., Member.]

Gramme. [*Vide* Breguet.]

Grollet. [*Vide* Camille-Grollet.]

* **Guillemin** [C. M.] Recherches sur la transmission de l'Électricité dans les fils Télégraphiques. 8vo. 19 pp. [Ext. des *Annales Télégraphiques*, Nov.—Dec., 1860.] *Paris*, 1860

* —— Note sur le nombre maximum de signaux Télégraphiques élémentaires qu'on peut transmettre, dans un temps donné au moyen de l'appareil Morse. 8vo. 4 pp. [Ext. des *Annales Télégraphiques*, Sept.—Oct., 1861.] *Paris*, 1861

Hughes. [*Vide* Collette.]

International Telegraph Convention. Revised at Rome, 1872. Translated by Alfred Brasher. Fo. 27 + 29 + 34 pp. *London*, 1872

—— Concluded at St. Petersburg, 1875. Fo. 70 pp. Translated by Alfred Brasher. *London*, 1875

—— With London Revision of Service Regulations and Tariffs, 1879. Translated by Alfred Brasher. Folio. 95 pp. *London*, 1879

Joubert. [*Vide* Allard and others.]

Krämer [J.] **und Lecher** [Dr. E.] Internationale Elektrotechnische Zeitschrift und bericht uber die Elektrische Ausstellung in Wien, 1883. 4to. 391 pp. *Wien, Pest, Leipzig*, 1883

* **Langlebert** [J.] Applications modernes de l'électricité. Nouvelles machines magnéto-électriques et dynamo-électriques, &c. 8vo. 104 pp. *Paris*, 1882

Le Blanc. [*Vide* Allard and others.]

Lecher. [*Vide* Krämer und Lecher.]

* **Lévy** [Maurice]. Sur les unités électriques. 8vo. 39 pp. [Soc. d'Encouragement pour l'Industrie Nationale. 4 Mars, 1882.] *Paris*, 1882

* **Maiche** [Louis]. Notice sur l'origine des transmetteurs téléphoniques et microphoniques. 4to. 20 pp. *Paris*, 1882

* —— Notice sur les piles électriques et leurs applications. La. 8vo. 24 pp. *Le Mans*, 1882

* **Malapert** [E.] Dimensions des Unités Electriques en fonction des unités fondamentales. (Centimètre—Gramme—Second). 8vo. 68 pp. *Paris*, 1882

* **Moerman** [Théophile]. Notice sur l'électro-métallurgie ou extraction économique et rapide des métaux précieux de leurs minerais: basé sur *l'emploi* de l'électricité pour tout faire. 8vo. 43 pp. *Bruxelles*, 1882

Moigno. [*Vide* Tyndall.]

* **Morel.** La Télégraphie. 8vo. 314 pp. *Paris*, 1884

* **Mourlon** [Charles]. L'Electricité à l'Exposition Internationale et Coloniale d'Amsterdam de 1883. La. 8vo. 44 pp. *Bruxelles*, 1883

Muirhead. [*Vide* Clark, Muirhead, & Co., Ld.]

N * * * [E.] [*Vide* Naves, E.]

* **Nansouty** [Max de] **et Audra** [R.] Exposition des Ministères de la Guerre et de la Marine. 8vo. 19 pp. ["Revue de l'Exposition Internationale d'Electricité." Groupe IV., Classe 6.] *Paris*, 1882

* **Naves** [E.] [E. N * * *] L'Electricité appliquée à la Guerre. 8vo. 123 pp. *Paris*, 1876

* ——— Le Réseau Télégraphique Français au point de vue de la défense des côtes et des frontières. 8vo. 16 pp. *Paris*, 1881

* **Olmeta** [J.] Instruction pratique pour l'emploi du chemin de fer et de la télégraphie en campagne. 8vo. 134 pp. *Paris*, 1872

* **Pelletier** [A. L.] **et D'Auge** [Taupin.] La pose et l'entretien des sonneries électriques mises à la portée de tout le monde ou manuel descriptif de tous les appareils constitutifs de la sonnerie électrique. La. 8vo. 72 pp. *Paris*, 1880

* **Perrodon** [J.] Téléphone-Avertisseur. 8vo. 7 pp. [Ext. de la *Revue d'Artillerie.*] *Paris*, 1879

* **Philippe** [M.] La Télégraphie aux Armées. 8vo. 63 pp. [Ext. du *Journal des Sciences Militaires*, Mars.] *Paris*, 1873

Philosophical Society of Washington. Bulletin of. Vol. VII. 8vo. lix. + 135 pp. *Washington*, 1885

* **Picou** [R. V.] Les Lampes Electriques. ["Revue de l'Exposition Internationale d'Electricité." Groupe IV., Classe 3.] La. 8vo. 75 pp. *Paris*, 1882

* ——— Manuel d'Électrométrie Industrielle. 8vo. 155 pp. *Paris*, 1882

* **Pilleux** [Lud.] Théorie Mécanique de l'Électricité. La. 8vo. 101 pp. *Paris*, 1881

* **Portevin** [H.] Les applications industrielles de l'électricité à l'exposition internationale de 1881. 29 pp. *Paris*, 1881

Potier. [*Vide* Allard and others.]

* **Pouget** [M. le Comte]. La Télégraphie Militaire par Signaux. 8vo. 16 pp. [Ext. du *Journal des Sciences Militaires*, Nov.] *Paris*, 1874

Preece [W. H.], F.R.S. Reports *re* Electric Lighting, 1883-84. 8vo. 92 pp. *London*, 1885

* **Prouteaux** [A.] De l'électro-magnétisme appliqué aux chemins de fer pour l'aimantation des roues motrices des machines locomotives afin de leur donner une adhérence additionnelle sans augmenter leur poids. 8vo. 14 pp. [Ext. du *Journal la Science Industrielle.*] *Paris*, 1881

Raillard. [*Vide* Tyndall.]

Rambusch [E.] Om Lynnedslag og Midlerne til at sikre sig mod de ved disse foraarsagede Ulykker. 4to. 16 pp. [Særtryk af den tekniske Forenings Tidsskrift.] *Kjobenhavn*, 1885 (?)
[Presented by R. von Fischer-Treuenfeld, Member.]

Rowell [G. A.] Electric Meteorology—What is Gas?—How the Theory was worked up. 8vo. 16 pp. *Oxford*, 1885

* **Romain** [A.] Manuels-Roret. Noveau manuel complet de la télégraphie électrique et de ses applications. 12mo. 386 pp. *Paris*, 1882

Roret. [*Vide* Romain.]

Salomons [Sir David], Bart. Four Photographs of Electric Light Installation at the residence of. 4to. 1885

Schäffler [O.] Elektro-automatische Ausschaltung und einstellung von Telegraphen und Telephon-Stationen. Für Erstere combinirt mit einer Einrichtung zum Gegensprechen. Beschrieben von Max Stipex. La. 8vo. 20 pp. *Wien*, 1884

Société des Ingénieurs Civils. Annuaire de 1885. 8vo. 196 pp. *Paris*, 1885
[By Exchange.]

South Australia [Reports]. [*Vide* Todd, Chas.]

Stipex, M. [*Vide* Schäffler, O.]

Telegraph Convention. [*Vide* International Telegraph Convention.]

* **Tissandier** [Gaston]. Le Probléme de la direction des aérostats—application de l'électricité à la navigation aérienne. 8vo. 32 pp. Plate. *Paris*, 1883

Todd [Chas.], C.M.G. Report on the Post Office, Telegraph, and Observatory Departments of South Australia. Fo. 283 pp. *Adelaide*, 1885

——— Meteorological Observations made at the Adelaide Observatory and other places in South Australia and the Northern Territory during the year 1882. Fo. 298 pp. *Adelaide*, 1885

Tresca [H.] [*Vide* Allard and others.]

* **Turner** [H. H.] A Collection of Examples on Heat and Electricity. 8vo. 75 pp. *London*, 1885

* **Tyndall** [John]. La Lumière: Notes d'un cours de neuf leçons sur le role scientifique de l'imagination. Traduit de l'anglais par M. l'abbé Moigno, accompagné d'un Appendice sur l'arc-en-ciel par M. l'abbé Raillard 8vo. 180 pp. *Paris*, 1872

* ——— Programme d'un cours en sept leçons sur les phénomènes et les théories électriques. Traduit de l'anglais par M. l'abbé Raillard, revu par M. l'abbé Moigno. Nouveau Tirage. 8vo. 72 pp. *Paris*, 1881

* **Urbanitsky** [P. d']. Les Lampes Electriques et leurs accessoires. Traduction et Appendice par G. Fournier. 8vo. 216 pp. *Paris*, 1885

Victoria. Report upon the Post Office and Telegraph Department, for 1882. By Graham Berry. Fo. 70 pp. *Melbourne*, 1883

Vienna Exhibition. Internationale Elektrotechnik Zeitschrift und bericht uber die Elektrische Ausstellung in Wien, 1883. 4to. 391 pp. *Wien, Pest, Leipzig*, 1883

* **Vinchent.** Mémoire sur les lignes télégraphiques du Royaume de Belgique leur matériel et leurs rapports avec l'exploitation des chemins de fer. La. 8vo. 40 pp. [Ext. des "Mém. de la Soc. des Ingénieurs Civils."] *Paris*, 1864

* **Vivarez** [Hy.] Construction des réseaux électriques aériens en fils de bronze silicieux lignes télégraphiques, téléphoniques, transport de force lumière électrique. 8vo. 175 pp. Deuxième Edition. *Paris*, 1885

Werdermann. [*Vide* Giffard.]

* **Wiedemann** [G.] Die Lehre von der Elektricität. Vierter Band. Erste Abtheilung. 8vo. 600 pp. *Braunschweig*, 1885

ORIGINAL COMMUNICATIONS.

ELECTRIC PRESSURE REGULATOR.

By Eug. Obach, Ph.D.

Vol. IX. of this Journal* contains an abstract of my paper on a laboratory apparatus designed for maintaining air or other gases in a confined space at any desired pressure. As then described, the apparatus was only suitable for pressures below the ordinary, but I had already indicated that no doubt it could be also arranged for compressed gases.

I now intend giving a brief description of the method employed for attaining this object, with comparatively slight alterations in the original construction. However, before doing so, I will just allude to the principle upon which this regulator is based, in order that the improvements mentioned hereafter may be more easily understood, referring the readers who wish to become acquainted with any details of construction of the original apparatus to my former communication.†

The apparatus consists of a siphon mercury barometer, the short limb of which is in communication with the space to be maintained at a given pressure, and is connected with a suitable air-pump. The barometer is so arranged that as soon as the mercury column reaches the desired position the current of a Leclanché cell is sent through an electro-magnet; the armature being attracted, a thin-walled india-rubber tube, which was previously tightly pinched, is released. As soon as the passage in the tube is unobstructed, gas rushes in until contact between the mercury and a platinum-pointed steel wire is broken. The air or other gas thus entering the apparatus, and re-establishing the required pressure, when the rarefaction exceeds that desired, passes through a bottle containing some non-volatile liquid, the bubbling of which shows that the apparatus is working properly.

---

* *Journal of the Society of Telegraph Engineers*, Vol. IX., p. 124. 1880.

† Carl's *Zeitschrift für angewandte Elektricitätslehre*, Vol. II., p. 69. 1880.

If strong sulphuric acid is used, it at the same time serves to dry the entering gases. By the insertion of a condenser, the sparks, otherwise caused by the frequent interruption of the current, are entirely subdued, and the surface of the mercury is kept perfectly clean and bright for a considerable time.

The alteration, in consequence of which the regulator can now be used for both high and low pressures, consists principally in the introduction of a commutator which, by simply turning a handle, arranges both the passages for the gas and the path for the current. This is of some importance, as any confusion which might possibly occur in the relative arrangement of the pneumatic and the electric connections might be detrimental to the apparatus: for instance, by blowing the acid from the bottle into the rubber tubes. I will not describe here the manner in which the passages are arranged so as to ensure in each case the passage of the gas in the right direction through the apparatus,* but shall confine myself exclusively to the electrical parts.

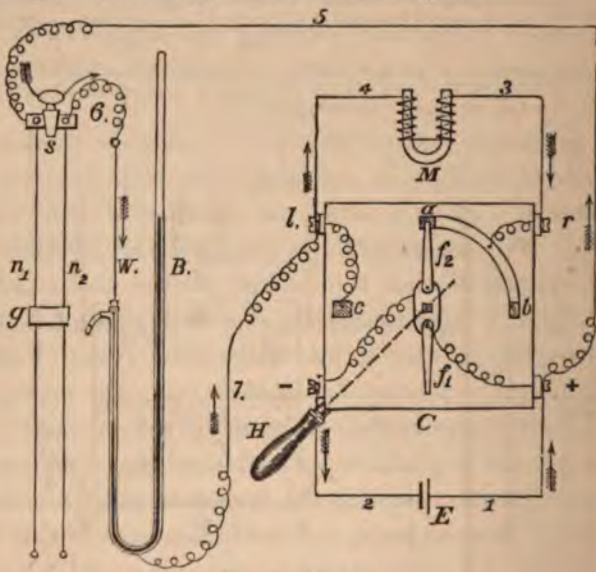


Fig. 1.—Vacuum.

Referring to Figs. 1 and 2, *B* represents the siphon barometer,

* A description of my pneumatic commutators will be given in Fresenius's *Zeitschrift für analytische Chemie*, Vol. XXIV., No. 4.

E the Leclanché cell, *M* the electro-magnet. The commutator is mounted on a square board, *C*, in which the brass quadrant *a b* and the square plate *c* are let in, the ends of *a b*, as well as the piece *c*, being faced with platinum. Two springs, f_1 and f_2, are fixed to a piece of insulating material, which can be turned on an axle in the centre, and at right angles to the board, by means of the handle *H*; the springs are in connection with the cell *E* by the wires 1 and 2. If the handle is to the *left*, as shown in Fig. 1, the connections are made for the maintenance of pressures below that of the barometer—*i.e.*, for *vacuum*. The current passes in the direction of the arrows from the cell through wire 5 and the stopper *s* to the steel wire *W*, which slides up or down, and can be fixed in any desired position. If the mercury rises sufficiently in the left limb of the barometer, it touches the platinum-pointed end of *W*, the current passing through the mercury to 7, then through the electro-magnet *M* to *a b*, the spring f_2, and back to the cell. The magnet attracts the armature, and allows the air to *enter* till contact is broken by the fall of the mercury. If the handle is to the *right*, as shown in Fig. 2, the regulator serves to maintain pressures above that of

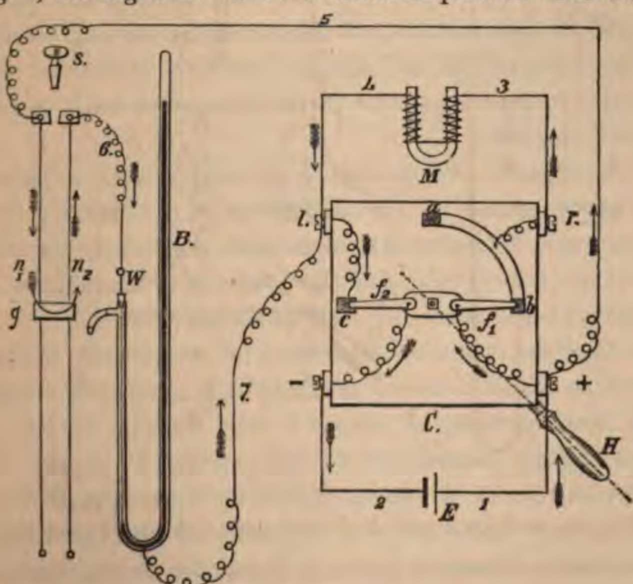


FIG. 2.—PRESSURE.

the barometer—*i.e.*, for *compression*. The stopper s being removed, the current from the cell E passes through the spring f_1 to the electro-magnet M, thence directly back to the cell through c and f_2. As long as the mercury is *not* in contact with the wire W the armature is attracted, and the india-rubber tube remains open, allowing the air or gas to *escape* until the pressure is sufficiently diminished, and the mercury makes contact with the wire. Part of the current now passes through the shunt, consisting of the barometer and a rheochord of two parallel German silver wires, n_1 n_2, electrically connected by means of a mercury sliding bridge, g. The latter is so adjusted that the shunt just weakens the current in the electro-magnet sufficiently to allow the spring to pull the armature off. The object of the rheochord is to introduce as large a resistance as possible in the circuit, in order to prevent unnecessary waste of battery power. For a higher pressure, and consequently a shorter mercury column between W and 7, a greater length of German silver wire can be inserted than is admissible for low pressures, and therefore longer mercury columns. The electro-magnet has about 10 ohms resistance.

The commutator is fixed in front of the vertical board carrying the electro-magnet, and the two German silver wires are stretched along the back of the tall upright board to which the siphon barometer is attached; neither therefore requires much space nor any special supports.

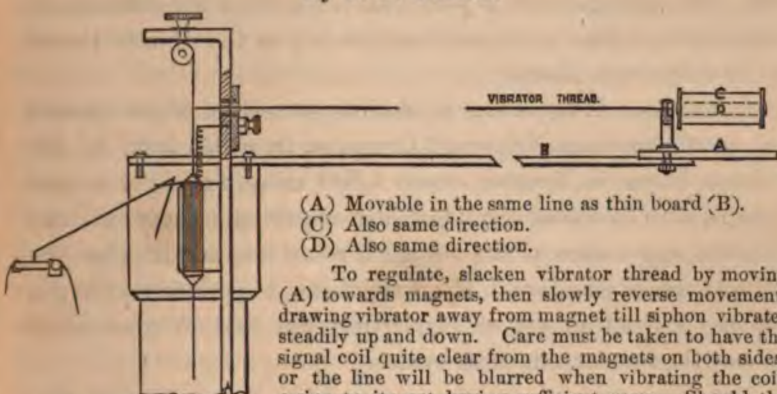
If the apparatus is but roughly adjusted, a kind of pumping action ensues, caused by the oscillations of the mercury in the barometer tube; but when the adjustment is carefully performed, the working is so gentle, and the pressure is maintained with such nicety, that scarcely any oscillations are noticeable.

The Electric Pressure Regulator, in its present improved form, will be found a useful auxiliary in a number of chemico-physical investigations, of which I will mention a few, viz.: firstly, the determination of the boiling point of a liquid under different pressures, or of various liquids at the same pressure (say 760 mm.), which latter can now be performed with equal facility whether the barometer is above or below the normal pressure; secondly, the fractional distillation of mixed liquids at various

pressures for the purpose of separation; and lastly, the maintenance of a constant pressure in any apparatus connected with a main from which the air is being constantly exhausted by means of a pump, or into which air is compressed, and which main is at the same time also occasionally used for other experiments.

MODE OF INCREASING THE SPEED AND DEFINITION OF SIGNALS WITH THOMSON'S PERMANENT MAGNET SIPHON RECORDER.

By S. F. Pescad.



(A) Movable in the same line as thin board (B).
(C) Also same direction.
(D) Also same direction.

To regulate, slacken vibrator thread by moving (A) towards magnets, then slowly reverse movement, drawing vibrator away from magnet till siphon vibrates steadily up and down. Care must be taken to have the signal coil quite clear from the magnets on both sides, or the line will be blurred when vibrating the coil, owing to its not having sufficient room. Should the vibrator not start well, it will most likely be found influenced by the large magnet, and should be placed a little further off. It is necessary to use condenser to prevent sparking at contact. The best siphons are long fine ones with only two bends.

Thinking it might be of interest to some of the members of your Society, I beg to send you the following account of a simple and very satisfactory arrangement for increasing the speed and definition of signals, with Sir William Thomson's Permanent Magnet Recorders, without electrification of the ink:—These recorders are admirable and economical for lines of 800 to 1,000 miles, but after that the friction between the siphon and paper begins to tell very much on the clearness of the signals. We have succeeded in reducing the friction to a minimum by vibrating the siphon very quickly, using a small make and break similar to that generally employed on induction coils. As will be known,

in these instruments the siphon is rigidly fixed to the suspended signal coil, therefore, to obtain vibration of the siphon, the whole suspension must vibrate. This is obtained by leading a fibre from the hammer of the make and break to the thread supporting the signal coil, and fastened to it about half an inch from the top. On the battery being applied, and the right tension given to the threads, the action is found to lift the siphon rapidly up and down, and the same fine line is obtained as in the mouse-mill recorders. The paper is adjusted so that when the vibrator is working the siphon is from one-sixteenth to one-eighth of an inch clear from the paper; the vibrator, being placed a foot from the magnets, is not affected by it, and can be cut off and started at will. The arrangement is placed at the back of the instrument, a hole being drilled in the centre-piece to pass the vibrator thread to the suspension thread.

It has been in operation on the longest section of the Central and South American Telegraph Company, from San Juan del Sur to Santa Elena, in Ecuador, nearly 1,600 miles, for eight or nine months, with excellent results, seldom requiring adjustment, and from the appearance of the signals it could not be told that the coil was under vibration. The line of ink is perfectly even; it also has the advantage of permitting the use of much finer siphons than under the usual mode.

Not having heard or read of anything being done in this direction previously, I trust my experiment may be something new.

San Juan del Sur, Nicaragua,
May 7th, 1885.

OBITUARY.

PROF. FLEEMING JENKIN, F.R.S.

We are indebted to the *Electrical Review* for the following memoir:—

"Our readers will join with us in the regret which we feel in performing the melancholy duty which devolves upon us this week of recording the death, which occurred at noon on Friday, the 12th June, of Prof. Fleeming Jenkin. Although the late professor had for some long time been in poor health, the consequence of overwork and much domestic anxiety, his most intimate friends entertained no misgiving as to the result of the comparatively trivial surgical operation which a week before he went to Edinburgh to undergo. The authorities of Edinburgh University, where he held the Chair of Engineering, had consented to release him from his duties for a twelvemonth, in order that he might recuperate his energies by travel and by residence on the Continent; and he had spent some little while in the South of England ere the journey back to Edinburgh for the purpose alluded to was undertaken. He looked forward with cheerfulness to the operation, and his confidence was in a measure shared by his friends. Blood-poisoning, however, set in, and, though it was not until the morning of Friday that imminent danger was apprehended, he died at noon on the same day.

"The deceased, the only son of Captain Charles Jenkin, R.N., of Stowting Court, Kent, was born in 1833. His mother was a native of Scotland, and the authoress of several popular novels. Inheriting both English and Scottish features of character from his parents, his mind and character were still further enlarged by the varied nature of his early education. He first attended the Grammar School at Jedburgh, and subsequently went to the Edinburgh Academy, where he had Clerk Maxwell for a schoolmate. When 13 years of age he was sent to a school at Frankfurt-on-the-Main, and, a year later, to Paris, where he remained

during the Revolution of 1848. Attending the University of Genoa subsequently to this, he there obtained the M.A. degree. In 1850 he commenced that engineering career which has since brought him so much distinction, his first experiences being gained in a locomotive shop at Marseilles. The following year he returned to England, and was apprenticed for three years at Sir W. Fairbairn's famous works in Manchester. He soon rose to honourable employments, his first important step being an engagement with Messrs. Newall, of Birkenhead, with whom, in the year 1857, he was engaged in making preparations for laying the first Atlantic submarine telegraph cable. This circumstance was the means of bringing him into close relationship with Prof. (now Sir) William Thomson, with whom there was soon formed a lifelong professional and intimate personal friendship, and from that gentleman he received much help and stimulus in his early professional career. Mr. Jenkin was successively engaged in the work of manufacturing not only the first Atlantic cable, but also the Red Sea cable, a cable extending from Singapore to Batavia, and several of the most important Mediterranean cables. He accompanied some of the expeditions for the laying of the latter. In the year 1861 he set up in business in London as a civil engineer on his own account, in partnership with Mr. H. C. Forde. Acting under the encouragement received from Sir William Thomson, he now began to write on scientific subjects, one of his earliest memoirs being a paper on 'The Transmission of Signals through Submarine Cables,' which was communicated to the Royal Society in 1862, and subsequently published in the *Philosophical Transactions*. On the motion of Sir William Thomson, a committee was named at the Manchester meeting of the British Association, held in 1861, for the determination of electrical standards. Mr. Jenkin was named as the reporter for that committee, and he was closely associated with Clerk Maxwell, and other able experimental investigators, in carrying out the work of the committee. For many years he recorded the work of the Electrical Standards Committee (much of it actually his own), and most of it was practically ratified at the International Conference of Electricians, held in Paris in 1883-84.

"For a couple of years, beginning in 1866, Mr. Jenkin was elected to the Professorship of Engineering in the University College, London. In that capacity he did such excellent work that he was fixed upon as the first occupant of a similar chair founded in the University of Edinburgh in the year 1868. He readily accepted the appointment when it was offered to him, and forthwith relinquished his London partnership with Mr. Forde. Professor Jenkin was not long in making an excellent position for himself in the University. A highly skilled and enthusiastic teacher, who was thorough and systematic in everything that he did, he gradually brought around him a number of able and accomplished young men, much of whose professional success in life is directly traceable to his influence. Several of them were Whitworth Scholars in Mechanical Engineering. Amongst his favourite pupils we may specially mention Professor Smith, of the Sir Josiah Mason College, Birmingham, and Professor Ewing, of the University College, Dundee. But the work of his classes did not use up the intellectual and physical energy of Professor Jenkin. Locally, he became connected with the Royal Society of Edinburgh, the Watt Institution and School of Arts, and other scientific institutions. Being within easy reach of Glasgow, he had much professional intercourse with Sir William Thomson, and as a result of that intercourse, as also with Mr. C. F. Varley and others, very great progress was made in the improvement of signalling apparatus for long submarine cables. He and Sir William Thomson were engaged as consulting electricians and engineers for a number of the most important submarine cables, and on several occasions he visited both North and South America in his professional capacity. Latterly he was joint engineer with Sir William Thomson of the Mackay-Bennett cables, and was also joint patentee with Sir William of important telegraphing instruments (specially for submarine cables), notably the automatic curb sender. His scientific aid was also in much request as a juror in international exhibitions. He was juror in physical apparatus at the London Exhibitions of 1862, in engineering at the Paris Exhibition of 1878, and in electricity at the Health Exhibition last year. Much of his success in carrying on scientific

projects in foreign countries was due to the fact that he was a highly accomplished linguist. German, French, and Italian were almost as familiar to him as his mother-tongue, and he was quite at home in French drama and literature.

"During the last three years of his life he attacked the problem of electric locomotion with characteristic energy and originality. Fired with the report of a lecture given at the Royal Institution in 1882, illustrating Professors Ayrton and Perry's absolute block system for preventing collisions and for dispensing with the aid of drivers, guards, and signalmen on electric railways, he proposed a scheme of joint working with these gentlemen for the development of a scheme of overhead automatic electric transport, to which he ultimately gave the name of *Telpherage*. The establishment of the Telpherage Company, of which he became one of the most prominent directors, was the next step, and after a long series of experiments had been conducted at Stevenage, in Hertfordshire, an actual telpher line, for conveying 250 tons of clay per week, for the Sussex Portland Cement Company, by means of iron buckets hanging on an overhead steel rod, supported on wooden posts, was undertaken. The manufacture of the electric locomotives to run along the steel ropes and pull the train of buckets after them, as well as the construction of the line, was engaging his attention at the time of his death.

"His originality and powers of suggesting devices for overcoming difficulties were prolific, as is shown by his manifold patents in connection with the details of telpher lines. Besides his numerous electric patents, he has made inventions in bridge work, hydraulics, mechanical methods of transmitting power, and heat engines.

"Neither his duties as a teacher, nor his multifarious occupations as a consulting engineer, an inventor, and a scientific witness and assessor at the Courts, were able to engross his activities. From 1859 onwards he wrote some fifty papers on scientific and social subjects, as well as several text-books. Of these last, the principal one—his book on Electricity and Magnetism in Messrs. Longmans' series of Text-books of Science—which has passed through many editions, and been translated into several foreign

languages, is generally accepted as the best existing elementary treatise on electricity. His scientific papers are for the most part to be found in the Transactions and Proceedings of the Royal Societies of London and Edinburgh, of the former of which he was elected a Fellow at the early age of 32, while of the latter he was made Vice-President in 1879. His papers exhibit much originality. One, published in 1878 (which gained the Keith Prize of the Royal Society of Edinburgh), lays the foundation of an entirely new dynamical theory of machines, which is as much in advance of the merely kinematical analysis of Reulaux as that was in advance of the fragmentary treatment which preceded it.

"Apart altogether from his scientific achievements, Professor Jenkin was in many ways a man of remarkable power, and even more remarkable variety. As an accomplished linguist, a litterateur, a critic, a metaphysician, a sportsman, no subject seemed to come amiss. Whether the talk was of technical education or the costume of the Greeks, of French plays or the freedom of the will, Professor Jenkin was sure to contribute a fact, a theory—a paradox, perhaps—but still something fresh and suggestive. Of him even more than of most men it might be said that only his intimate friends really knew him, and his intimates were for the most part specialists in literature or art, or in anything rather than the subjects that formed the main business of his own life. Of his literary work the pages of the *North British Review* for 1868 contain examples, among which an article on the Atomic Theory of Lucretius may perhaps be specially named. Other of his writings treat of Darwinism, education, economics, and hygiene. In this last subject he did more and better work than merely that of the pen. In 1877 he organised the Edinburgh Sanitary Protection Association, a co-operative society which secured for its members the benefit of periodical inspection and skilled advice regarding drainage. The idea was novel, and a man of less energy would have found the opposition and prejudice of the public strong. He soon launched the Edinburgh Association on a career of assured prosperity, and then promoted the formation of similar societies in London and other places. A like society was founded in Dundee a little more than a year ago, of which, as of many of the others,

Professor Jenkin was honorary consulting engineer. With other titular distinctions, Professor Jenkin was a LL.D. of Glasgow University, and an honorary member of the Société Professionelle d'Hygiène of Paris. He leaves a widow, the only daughter of the late Mr. Alfred Austin, to whom he was married in 1859, and three sons, the eldest of whom, Austin F. Jenkin, has had a distinguished career at Trinity College, Cambridge."

ABSTRACTS.

J. RAYNAUD—METHOD OF DIMINISHING THE DANGERS DUE TO THE EXTRA CURRENT PRODUCED IN DYNAMO MACHINES ON BREAKING THE OUTER CIRCUIT.

(*Comptes Rendus, Vol.* 100, *No.* 9, 1885, *p.* 633.)

In a former sitting of the Academy, Mr. d'Arsonval suggested the interposition of a polarisation battery between the terminals of the dynamo machine. The author proposes to fall back upon an arrangement devised from Faraday's principle of lateral induction, in use in all telegraphs under the name of "lightning discharger." It would be sufficient to join up such an apparatus to the terminals of the dynamo, when it would only allow the current to pass when this became dangerous. According to circumstances, various forms of lightning dischargers might be used, with points, or paraffined paper, or alcohol. The experiments of Warren de la Rue and Müller would serve to determine the striking distance in air at ordinary pressure and between various surfaces for differences of potentials from 250 to 1,200 chloride of silver cells, each of which may be taken as having the value of 1·03 volt.

H. PELLAT—METHODS OF DETERMINING THE POTENTIAL OF THE AIR.

(*Comptes Rendus, Vol.* 100, *No.* 10, 1885, *p.* 735.)

The first point investigated was the rapidity with which the apparatus generally used is sensible to changes of potential. A large sheet of metal could either be brought to zero potential by connecting it to the system of gas pipes, or charged to 100 volts. The readings were made with a Mascart's electrometer, the quadrants of which were respectively at the potentials + 50 volts and − 50 volts, the needle being connected to the point to be tested. It was found that all water-dropping apparatus were slow in acting; thus, with a flow of 12 litres in 12 hours, it took five minutes for the needle to be brought to the potential of the air.

The use of smoke arising from the burning of filtering-paper saturated with lead nitrate is still more slow, and presents an important source of error, as the combustion itself charges the electrometer to a potential differing as much as 8 or 10 volts from that of the air.

As a result of these experiments, the author was led to adopt a gas flame burning *from a metal burner*, carefully insulated and connected to the electro-

meter. At the very first it was found that by this means the needle was brought almost instantaneously to the air-potential.

In order to study the E.M.F. which might be produced by the combustion, the burner was placed within a hollow metallic cylinder closed at the top, with the exception of a few holes to cause an up-draught. Such an arrangement behaves exactly like a galvanic cell, the burner being the positive pole, and the envelope the negative. The value of the E.M.F. depends, 1st, on the gas burnt; 2nd, on the metal of the burner; 3rd, on the nature of the internal surface of the envelope. Some of the values obtained may be of interest.

Gas.	Burner.	Envelope.	Volts.
Hydrogen	Brass	Copper	0·30
Do.	Do.	Zinc	0·58
Do.	Zinc	Copper	0·09
Do.	Platinum	Do.	0·45
Do.	Do.	Platinum	0·10
Coal Gas	Do.	Do.	0·94
Do.	Do.	Copper	1·72

The resistance was also measured by the duration of the charge of a Leyden jar, and it was found that with a coal gas flame one centimètre high, burning in an envelope thirteen centimètres in diameter, the resistance was 69,000 megohms.

E. BICHAT and R. BLONDLOT—DIFFERENCES OF POTENTIAL BETWEEN LIQUIDS, AND THE ACTION OF THE AIR IN THE MEAUSREMENTS OF THESE DIFFERENCES.

(*Comptes Rendus, Vol.* 100, *No.* 11, 1885, *p.* 791.)

Mr. Lippmann has established two distinct equations relating to the properties of the surface-contact between mercury and an electrolyte; and Helmholtz has demonstrated that it results from one of these equations that if, by a suitable polarisation, the capillary constant of the surface-contact has been brought to its maximum value, the mercury and the electrolyte are at the same potential.

The authors have carried out a series of experiments founded on this principle. In a capillary electrometer the acidulated water was replaced by the electrolyte to be experimented upon, and by successive trials the E.M.F. was determined which had to be inserted between the terminals of the instrument to obtain the maximum rise of the meniscus in the capillary tube; at this moment the difference of potential in the tube was *nil*, and the interposed E.M.F. exactly measured the normal E.M.F. The method, therefore, is, so to *speak, a zero* one, and—what is most important—it is one in which the air

does not come into play at all. It is easy to see that by measuring as explained the differences of potential mercury | electrolyte No. 1 and electrolyte No. 2 | mercury, it is possible to arrive at the difference of potential electrolyte No. 1 | electrolyte No. 2.

From a comparison of the values obtained by the above method and by that usually employed, the important part played by the air is very apparent. Thus, with acidulated water and sulphate of soda, the old method gave $+0{\cdot}129$, the new $-0{\cdot}20$; for sulphate of soda and sulphate of potash, the old gave $-0{\cdot}138$, the new $+0{\cdot}475$. This difference can only be explained by the fact (foreseen by Maxwell) that between the air and a liquid there exists a difference of potential the value of which varies with the liquid.

A. GAIFFE.—GALVANOMETERS WITH CURVED SCALES

(*Comptes Rendus, Vol.* 100, *No.* 11, 1885, *p.* 794.)

The wire is wound in grooves and forms, above and below the needle, in planes, which are parallel to its planes of oscillation, two figures resembling the caustics which are produced by the reflection of light from a cylindrical surface. In consequence of the shape of these curves, the magnetic axis of the needle, whatever may be its direction, always cuts the neighbouring part of the scale at very nearly the same angle; the closeness of the needle and of the coil, and consequently their reciprocal action, increase at the same time as the action of the magnetic couple of the earth. By this means it is possible to construct instruments in which, for currents increasing in arithmetical progression, the needle has its positions of equilibrium equidistant on the two halves of the scale to about 70 degrees from the zero point.

CLAMOND and J. CARPENTIER—A NEW FORM OF THERMO-PILE.

(*Comptes Rendus, Vol.* 100, *No.* 15, *April* 13, 1885, *p.* 985.)

The couples consist of plates either of iron or nickel, and of bars of an alloy of antimony and zinc. The maximum of effect is obtained when the two metals of the alloy are mixed in the proportion of their chemical equivalents (Sb = 122; Zn = 65). Each couple attains its maximum of effect at the fusing point, when the couple iron-alloy has an E.M.F. of $\frac{1}{10}$ of a volt, and the couple nickel-alloy $\frac{1}{8}$ of a volt. In ordinary work the temperature is not allowed to rise so high, and at the usual working point the E.M.F. is $\frac{1}{14}$ of a volt and $\frac{1}{12}$ of a volt respectively. The framework of the pile is formed of baked clay, having a central cylindrical tube with partitions radiating from it; the plates of iron or nickel having been placed suitably in the cells, the alloy is poured in when molten; and hence, should the temperature rise too high at any time during the working, the only effect will be to fuse this alloy, which will again solidify on cooling without any damage having been done to the pile.

Two types of pile have been constructed, one with 120 couples arranged

in 12 circles superposed, which has an E.M.F. of 8 volts, and an internal resistance of 3·2 ohms; the other with 60 couples in 6 circles, which has an E.M.F. of 3·6 volts, and an internal resistance of 0·65 ohms. The consumption of gas is the same for both types, viz., 6½ cubic feet per hour.

MEASUREMENT OF SMALL RESISTANCES.

(*L'Electricien, Vol. 9, No.* 101, *March* 21, 1885, *p.* 210.)

In the first appendix to the fifth report of the Committee of Electrical Standards is a description of an instrument devised by the late Sir Wm. Siemens for the measurement of small resistances. The principle of this apparatus is that the current from the battery follows two parallel circuits, the one comprising a coil and a known resistance, the other a similar second coil and the unknown resistance. Between the two coils, which themselves are of equal resistance, is a needle, and the two coils can be displaced laterally in the one direction or the other so as to bring the coil through which the least current flows nearer to the needle, which then, being equally affected by both coils, remains at zero. In the Siemens "Resistance-Measurer" the movement of the coils was effected by moving a curved piece of metal at right angles to their motion; one end of the rod carrying the coils is pressed continuously against the curved rule by a spring, and therefore has to follow its motion.

The new instrument, the "Microhm-meter," which Mr. Maiche has lately brought before the Société Internationale des Electriciens, is, in some details, an improvement on the older form. The two coils are no longer rigidly connected, but each is carried on its own screw, and can be moved independently of the other. Thus the fixed comparison resistance is done away with, as well as the curved guide. Each complete turn of the screw corresponds to a movement of one millimètre; one of the micrometer wheels, which is attached to the screw, is divided into a hundred parts, and turns in front of a vernier. With a single Daniell cell a displacement of a quarter of a degree is perceptible. The value of the movement of the coils is so arranged that each degree corresponds to one-thousandth of an ohm.

E. REYNIER—THE SWELLING OF THE LEAD IN SECONDARY BATTERIES.

(*L'Electricien, Vol.* 9, *Nos.* 104-5, *April* 11 *and April* 18, 1885, *pp.* 261 *and* 277.)

In these two articles the author treats of the changes of volume of the lead plates during the formation and during the discharge, resulting from changes of weight and density. Before the formation both plates consist of compact metallic lead. When the cell has been formed and charged, the positive plate has become partially peroxidised, and, since this peroxide is lighter and less dense than the lead, it occupies a greater volume. The negative plate does not vary sensibly in weight, but it has become spongy; hence an increase of volume on this side also. On discharging the cell, sulphate of lead is formed, which is lighter and less dense than either the peroxide or

the spongy lead, leading to a further increase in volume. On recharging, the volumes decrease. Thus the volumes of the lead plates increase when they pass into the condition of formed and charged electrodes; they still further increase during the discharge, and diminish during the recharging, but not so much as to return to their original volume. In Planté's form of battery, and allowing for only a partial formation of the plates, the author finds for the cubical swelling of the positive plate the value 1·05, and for the negative 1·045. These figures show that this increase in volume is not negligible, and may produce mechanical effects. In Faure's accumulators, on the other hand, the swelling is, so to speak, negative in sign, and therefore these accumulators do not show signs of mechanical action. In Planté's cells a mean expansion of about 4 per cent. of each plate occurs in each of its dimensions during the discharge. In one of the spiral form, having electrodes 0·50 m. long, the increase in length is as much as 2 cm. But this increase is not regular throughout the mass; the portions least acted on expand least, and hence the buckling of the plates is caused.

The author concludes that the permeable material ought to be thinly spread, and over large surfaces, and the supporting plates should be to a certain extent elastic, to allow of these changes of volume to take place more readily; and he therefore proposes corrugated plates, which seem to meet these requirements.

Dr. A. D'ARSONVAL—SIMPLE GALVANOMETERS.

(*La Lumière Electrique, Vol.* 15, *No.* 10, *March* 7, 1885, *p.* 461.)

In carrying out his researches on electro-physiology Dr. d'Arsonval had need of some very sensitive galvanometers, and he describes several forms which can be readily constructed with a few pieces of iron wire, some needles, and a few bobbins.

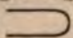
1. A straw, to which is attached a mirror, is hung horizontally from a fibre; at one end of the straw is fixed a needle point, at the other a counterpoise. On the point of the needle a small iron bar can rotate in a horizontal plane; and each end of the bar can traverse the hollow of a small coil of wire. The directive force of the earth obliges the iron bar to take up a fixed position on its pivot, but it exerts no force on the horizontal straw, which is only solicited by the torsional force of the fibre.

2. A new form of astatic needles is arrived at by fixing two small pieces of steel (a piece of watch spring), bent into a U shape, back to back, with opposite poles at the top. The whole is hung from a fibre, and has a mirror attached.

3. This instrument is a sort of Thomson galvanometer in which the suspension is done away with. Two sewing needles are fixed horizontally across the back of the small mirror, and the points of the two needles rest on the end of a horizontal magnet. The whole being placed in a coil of wire, a very sensitive galvanometer is obtained, the needles turning about their points.

4. Two sewing needles are passed through a piece of cork, and are placed *vertically on their points* on the ends of a horse-shoe magnet, with its poles

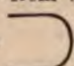
vertically upwards; a counterpoise is fixed below the cork between the two legs of the magnet. The mirror is fixed horizontally on the top of the cork, and the light is reflected by means of a prism. The two upper ends of the two needles are bent back at right angles, and the bent portions enter the hollows of two solenoids. Thus the whole movable portion pivots on the points of the needles, and the instrument is of course extremely sensitive. Its sensibility can be adjusted by moving the counterpoise up or down.

5. A steel wire is magnetised and bent into a U shape, the ends being bent back at a right angle. The whole is suspended from a fibre so that the wire hangs thus  in a vertical plane, and the bent-back points pass through two solenoids.

6. A very similar arrangement, but the bent steel wire hangs altogether in a horizontal plane, and the ends enter opposite sides of the same bobbin, there being only one instead of two.

C. MENGES—A NEW FORM OF ASTATIC GALVANOMETER.

(*La Lumière Electrique, Vol.* 15, *No.* 12, *March* 21, 1885, *p.* 543.)

On a wooden base provided with levelling screws are placed two upright solenoids side by side; these can be changed as occasion requires. At the back is a vertical support from which the needle is hung by a fibre. The needle itself is of this form , and is suspended in a stirrup at a point near the vertical connecting part. The horizontal arms swing just above and just below the top and bottom of the solenoids, which act on the needle laterally. The disposition of the needle corresponds closely to that in Thomson and Gray's instrument.

R. WEBER—THE ELECTRICAL SIREN.

(*Annalen der Physik und Chemie, Vol.* 24, *Pt.* 4, *No.* 4, 1885, *p.* 671.)

In its simplest form this instrument consists of a metallic toothed wheel fixed on a metal axle; on the edge of the wheel a flat spring presses, so that it lies alternately on a tooth and a space. These spaces are all filled in with an insulating material. From the fixed end of the spring a wire goes to a battery, and the circuit is completed through a telephone to the axle of the wheel, and is completed or broken accordingly as the spring touches a tooth or an insulated space. The pitch of the note heard in the telephone will be proportioned to the number of teeth in the wheel and to its speed. The loudness will depend on the strength of the current and on the kind of telephone used. The colour of the note (timbre), *i.e.*, the number of overtones, will be influenced by the constancy of the battery, and the kind of telephone.

The actual siren is made up of several toothed wheels with a varying number of teeth mounted side by side on the same axle, but insulated from each other. The corresponding spring for each wheel is connected to a *separate binding* screw. A battery of several cells is used, all the negative

plates being connected up parallel and joined to the axle, while each positive plate goes to a separate binding screw; according to the number of wires which are joined up one or more wheels will be in action, and several notes can be compounded, all of which will be heard together in the telephone. The wheels can be conveniently driven by a small electromoter keyed on to the end of the axle. The counting is effected by an endless screw at the other end gearing into a wheel with 150 teeth; to the vertical spindle of this wheel is attached a dial plate with 400 divisions, over which are two hands, which are normally held by catches, but which can be released at any desired moment. At a given moment one hand is released and then moves round with the dial; after a given interval of time the second is released, and then also goes round with the dial. From the distance apart of the two hands, the interval of time, and the number of teeth, the time of vibration can be reckoned. The author found that the relative breadth of the teeth compared with breadth of the insulating parts did not affect the primary tone produced, but that it had considerable effect on the overtones produced along with it. A particular experiment was tried with four wheels each with 40 teeth, but with the following ratios of metal to insulator:—1st, 1 to 12; 2nd, 3 to 12; 3rd, 6 to 12; 4th, 9 to 12. The first wheel gave the double octave, but the octave itself could not be distinguished; the second gave several overtones, a very feeble octave, the upper fifth very clearly, as well as the double octave, and also three harmonic overtones—1:5, 1:6, and 1:8; the third wheel gave the upper fifth and the octave; the fourth, the two octaves. The experiments were carried out altogether with twelve wheels, with teeth varying in number from 24 to 72; and the paper concludes with a list of the tones produced by various combinations of these.

A. BARTOLI—RELATION BETWEEN CONDUCTIVITY AND COMPOSITION OF VARIOUS KINDS OF CARBON.

(*Beiblätter, Vol.* 9, *No.* 3, 1885, *p.* 172.)

By the continued action of a red heat the conductivity of all calcined organic substances increases from zero, and at the same time the amount of hydrogen in their composition diminishes. At still higher temperatures the conductivity increases still more, and the hydrogen always decreases; thus, when the hydrogen decreased from 4·2 to 0·175 per cent., the specific conductivity rose from 0 to 0·012. If finely-powdered graphite is intimately mixed with 20 per cent. of paraffin, the compound conducts readily; but if 30 per cent. of paraffin is used, the conductivity becomes *nil*. The less finely the graphite is powdered the better the mass conducts.

A. BARTOLI—CONDUCTIVITY OF COMPOUNDS OF CARBON.

(*Beiblätter, Vol.* 9, *No.* 3, 1885, *p.* 172.)

The solid substances, the insulation of which was to be tested, were brought between two metal plates 1 cm. in diameter, one of which was connected *to one pole of the battery*, the other to a galvanometer. Of two other

metal plates separated by the body under test, one was joined to the other terminal of the galvanometer, while the other could be connected to the battery. In the liquids thin vertical platinum wires were hung 1 mm. apart. As battery 400 bichromate cells were used, as well as 400 copper zinc couples in sodium nitrate. With petroleum and similar substances, the loss of electricity of a metal ball was measured.

All solid carbon compounds are non-conductors. If a conducting body is dissolved in a non-conducting liquid, the solution conducts. If a non-conducting liquid remains non-conducting, even after the solution of a solid in it, then the solid body is a non-conductor in the liquid state.

A non-conducting liquid remains the same with increase of temperature; a conductor increases its conductivity on cooling.

In general, the conductivity diminishes in homologous series with the increase of complexity of the electro-positive radicals.

H. KAYSER—MEASUREMENTS OF NOË'S THERMOPILE.

(*Beiblätter, Vol.* 9, *No.* 3, 1885, *p.* 181.)

With two batteries of 20 to 25 elements, the E.M.F. rises at first proportionately to the consumption of gas, and reaches 2 volts; it is very constant. The resistance increases with the consumption of gas up to 1 ohm, then decreases to $\frac{3}{4}$ ohm, and is very variable. With an external resistance not exceeding 5 ohms, the current from thermopiles is cheaper than that of the Bunsen cells.

A. ZILLICH—DIRECT MEASUREMENT OF RESISTANCE OF CONDUCTORS.

(*Zeitschrift für Elektrotechnik, Vol.* 3, *No.* 5, *March* 15, 1885, *p.* 137.)

A Weber's tangent galvanometer is provided with two coils, which can either be placed on the same side or on opposite sides of the needle. The one coil consists of a single turn of wire of very small resistance, r_1; the other is made up of from 10 to 50 turns of wire of resistance r_2. The resistance to be measured, x, is joined up to the first coil, and a rheostat to the second coil. Both coils are connected up parallel in such a way that the currents in them are opposed as regards their action on the needle. If now so much resistance, r_0, is inserted in the rheostat that the needle stands at zero, we have

$$x = \frac{K_2}{K_1}(r_0 + r_2) - r_1,$$

where K_2 and K_1 are the reduction factors of the galvanometer coils, and

$$K_2 = \frac{C_2 H}{2\pi n_2} \qquad K_1 = \frac{C_1 H}{2\pi n_1},$$

C_2 *and* C_1 *being* the radii of the coils, and n_2 and n_1 the number of turns.

H. VIVAREZ—USE OF SILICIUM BRONZE WIRE FOR SUBMARINE CABLES.

(*Zeitschrift für Elektrotechnik, Vol.* 3, *No.* 7, *April* 15, 1885, *p.* 210.)

The author most strenuously advocates the use of silicium bronze wire for the above purpose on account of the lightness which can thereby be obtained in those cables. The conductivity does not differ materially from that of commercial copper, while the breaking strain is very much higher, consequently the conductor itself can be relied upon to take much of the strain, and the iron wire sheathing may be made much lighter; thus leading to increased facility in all repairing operations as well as in the laying of the cable. He contrasts the cable laid for the French Atlantic Company with one of silicium bronze, and a statement of this comparison will bring out the salient points on which he founds his advocacy of the latter kind of cable. Taking only the deep-sea cable, we have the following weights for the French Atlantic Cable, per knot:—

Copper conductor	485 lbs.
Guttapercha	397 „
Jute serving	176 „
18 iron wires 2 mm. diameter	1,896 „
Hemp and compound	882 „
	3,836 lbs.

The total diameter of the cable is 30 millimètres, its breaking strain 6,614 lbs., and its weight in sea-water 992 lbs. It will carry from 6 to 7 miles of its own length.

Now take a deep-sea cable with silicium bronze conductor, per knot:—

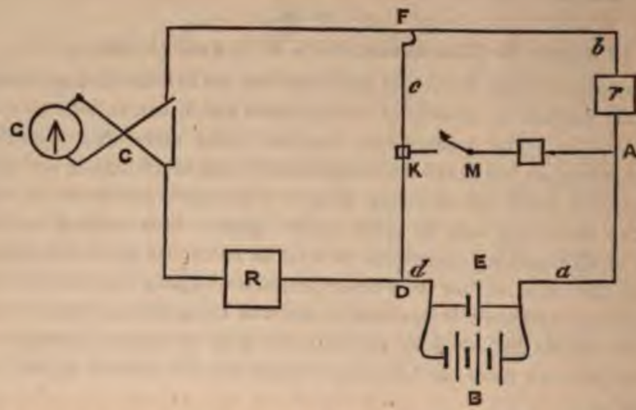
Silicium bronze conductor	485 lbs.
Guttapercha	397 „
Jute serving	176 „
28 iron wires, each 1·25 mm. diameter	1,102 „
Hemp	551 „
	2,711 lbs

The total diameter will be 25 millimètres, its breaking strain 6,173 lbs. (of which about half will be carried by the conductor itself), its weight in sea-water 705 lbs. It will carry from 8 to 9 miles of its own length—that is eight or nine times its own weight in water.

C. MICHALKE—MEASUREMENT OF THE DISCHARGE OF SECONDARY BATTERIES.

(*Electrotechnische Zeitschrift, Vol.* 6, *No.* 4, 1885, *p.* 149.)

By an alteration of Mance's the author has made an arrangement by which the current and internal resistance can be readily measured. The accompanying figure shows the arrangement of the apparatus.



E is the secondary battery, and B the charging battery, which can be joined to each other by a commutator, or the secondary battery can be joined to the measuring circuit. By lifting out a copper bridge-piece at F, the connection of the bridge wire with the rest of the circuit can be interrupted. D F is a German silver wire, 2 m. long and 1 mm. thick, stretched along a scale divided into millimètres; and along this wire the contact-piece K can slide. r is a resistance of about 0·2 ohm, R is a resistance box, and a third resistance of about 2 ohms is inserted in the wire K A so as to prevent too rapid discharge of the accumulator.

If D F = x millimètres, F K = y millimètres—these distances having been attained by moving K until no alteration in the deflection is produced when the key M is closed—then

$$\frac{R_0}{r} = \frac{x - y}{y},$$

where R_0 is the internal resistance of the battery. If r_1 is the resistance of the wire D F,

$$I = i_1 + i_1 \left(\frac{R}{r_1}\right),$$

where i_1 is the current in the galvanometer circuit. And

$$E = I\,(r + R_0) + i_1 R$$
$$= i_1 R \left(\frac{r}{r_1} \cdot \frac{x}{y} + 1\right).$$

Dr. F. KOHLRAUSCH—USE OF THE WATER VOLTAMETER FOR DIRECT MEASUREMENT OF STRONG CURRENTS.

(Elektrotechnische Zeitschrift, Vol. 6, No. 5, 1885, p. 190.)

After glancing at some of the objections to the use of silver or copper voltameters, the author goes on to describe a large form of voltameter which *he* has found very useful for the direct measurement of currents up to *100 ampères.* The water voltameter introduces a counter E.M.F. of about

2 volts into the circuit, but this quantity is tolerably constant, while the internal resistance may be reduced to $\frac{1}{50}$ of an ohm.

A glass tube 40 mm. in diameter, and capable of holding 200 c.cm., is divided into spaces each of 5 c.cm. The bottom part of the tube is tapered and fits into one neck of a large heavy glass vessel, which contains about 500 c.cm. and serves as foot to the apparatus; a second neck serves to place the vessel in communication with the air during an experiment. At the top of the tube a small thermometer is sealed in, to permit of the temperature being read. The electrodes are of platinum sheet. One consists of a single sheet 17 mm. by 40 mm., and it is placed between two equal sheets, which together form the second electrodes. The platinum electrodes are united to platinum tubes, which pass through lateral tubulures towards the bottom of the voltameter tube. The liquid used is a 20 per cent. solution of sulphuric acid in water (specific gravity = 1·14).

In order to attain to an accuracy of one-half per cent. the temperature must be known within one degree, and the pressure must be ascertained with 4 mm. of mercury.

If h is the height of the dilute acid in the tube above the level of the acid in the vessel, we have to subtract $\frac{1}{12}h$ from the height of the barometer (b) to find the pressure on the evolved gases in the tube. (14·1 is specific gravity of the acid; and 13·6 is specific gravity of mercury—thus, $\frac{1 \cdot 14}{13 \cdot 6} = \frac{1}{12}$.)

It is found that at a temperature of 20° C, and a pressure of 725 mm. of mercury, 1 ampère in 1 second liberates $\frac{1}{5}$ c.cm. of gases. It is easy to construct a table of corrections (given in the original article) for various temperatures and pressures, and then the calculation of the current is very simple, as an example will show:—

Height of barometer	754 mm.
Height of acid...	112 mm.
Pressure = $754 - \frac{112}{12}$ = ...	745 mm.
Temperature	17·8° C.
Volume of gases measured	198·0 c.cm.
Correction from table	+ 7·5
	205·5 c.cm.
Time	39 seconds
Volume per second	$\frac{205 \cdot 5}{39}$ = 5·27 c.cm.
Current...	5·27 × 5 = 26·3 ampères

Dr. M. T. EDELMANN—CALIBRATION OF GALVANOMETERS.

(*Elektrotechnische Zeitschrift, Vol. 6, No. 5, 1885, p. 194.*)

The author is of opinion that not sufficient care is taken in the calibration of the present direct-reading instruments, and that it is impossible to put any trust in an instrument with a graduated scale. He objects to the law of tangent deflection being regarded as correct for anything beyond very small

deflections. Galvanometers, however much they may be alike in construction, size, resistance, &c., cannot be so made as to have all of them the same constant, and therefore dials graduated in the same way cannot be used indiscriminately for a batch of instruments of the same construction.

LIST OF OTHER ARTICLES.

(*Comptes Rendus, Vol.* 100.)

No. 9.—**T. JOB**—Memoir on the Origin of Atmospheric Electricity. **L. DESCROIX**—Maxima of Diurnal Variations of Earth's Force in 1882 at Observatory of Mont Souris. **A. DAUSSIN**—Claim of Priority for Discovery of Method of Preventing the Extra Current in Dynamo Machines.

No. 10.—**MASCART**—Determination of Ohm by the Method of Damping. **E. GAILLARD**—A New System of Dynamo Machines. **A. DUPRÉ**—Cell with Two Liquids. **T. MOUREAUX**—Diurnal Variations of Earth's Magnetic Force observed at Parc St. Maur.

No. 12.—**VULPIAN**—Experimental Researches on the Electrical Excitability of the Brain.

No. 15.—**A. RUHELL**—The Electrical Problem from the Point of View of Economy and Efficiency of Dynamos.

(*L'Electricien, Vol.* 9.)

No. 100.—**E. HOSPITALIER**—Exhibition of the "Société des Electriciens." **E. ARNOULD**—Gas Lighters, and Leak Testers. Testing Office for Standards of Resistance.

No. 101.—**E. HOSPITALIER**—See *ante.* **COLSON**—New Magnetic Telephone.

No. 102.—**E. HOSPITALIER**—See *ante. Anon.*—Telegraphic Communication with Trains in Motion.

No. 103.—**L. GREZEL**—Electricity at Bellegarde. **Dr. B. DU ROCHER**—New Galvano-caustic Cell.

No. 104.—**E. HOSPITALIER**—See *ante.*

No. 105.—**E. HOSPITALIER**—See *ante.*

(*La Lumière Electrique, Vol.* 15.)

No. 9.—**G. COLOMBO**—Central Station at Milan. **F. UPPENBORN**—Lighting of some Streets in Hanover.

No. 10.—**C. DECHARME**—Application of Electricity to the Study of the Vibrations of Solids and Liquids. **E. VAN DEN VEN**—Oxide of Copper Battery.

No. 11.—**J. BOURDIN**—Printing Telegraph of Baillehacher. **A. SOUBEYRAN**—Electric System of Signals at the Marles Mines. **L. DE LOCHT-LABYE**—Telephony by Discontinuous Currents. *Anon.*—Electro-Deposition in Mints.

No. 12.—**A. GUÉROUT**—Electric Lighting by Primary Batteries. **P. SAMUEL** — Installation of Telephone Lines in Workshops. **DELALANDE and CHAPERON**—Consumption of Zinc in Oxide of Copper Batteries. **J. LUVINI**—Formation of Hail. **LASSANCE**—Commutator for Intermediate Telephonic Post. **E. EDLUND**—Researches on the Behaviour of Electricity in Rarefied Air.

No. 14.—**J. BOULANGER**—The Absolute System of Units.

No. 15.—**C. DECHARME**—Application of Electricity to the Study of Vibratory Forms. **J. LUVINI**—Origin of Atmospheric Electricity.

No. 16.—**A. GUÉROUT**—Winding of Armatures of the Siemens Type.

(*Annalen der Physik und Chemie, Vol.* 24.)

No. 3.—**G. QUINCKE**—Electrical Researches.

No. 4.—**E. KITTLER**—Measurement of Currents. **G. QUINCKE**—See *ante*. **A. GOCKEL**—Relation of Peltier Effect to the Efficiency of Cells. **W. H. SCHULTZE**—The Mutual Action of Two Magnetic Particles at Right Angles to each other.

(*Beiblätter, Vol.* 9.)

No. 3.—**L. PALMIERI**—Researches on Electric Potential. **A. RIGHI**—New Explanation of Hall's Phenomenon. **E. FOSSATI**—Behaviour of some Permanent Magnets in presence of a Keeper. **F. MILLER**—Apparatus for showing Lines of Force in a Pacinotti Ring. **P. CARDANI**—The Duration of Retarded Discharges. **E. WARBURG**—Phosphorescence in Geissler's Tubes.

(*Elektrotechnische Zeitschrift, Vol.* 6.)

No. 4.—**Von HEFNER ALTENECK**—New Arc Lamp. **A. BERINGER**—Kapp's Measuring Instruments. **P. N.**—Use of Bichromate of Soda in Cells. **P. NIPKOW**—Small Accumulators for Telegraph and Telephone Lines.

No. 5.—**ELSASSER**—The Van Rysselberghe System. **HUMMEL**—Unipolar Machines. **Dr. R. RÜHLMANN**—Arc Lamps for Small Currents. **P. N.**—Chaudron's Thermopile.

(*Zeitschrift für Elektrotechnik, Vol.* 3.)

No. 5.—**Dr. S. DOLINAR**—New Method of Winding Gramme Ring.

No. 6.—**Prof. GOPPELSRÖDER**—Action of Electrolysis on Indigo. *Anon.* Loss of Energy in Electric Lighting. *Anon.*—Watt and Horse-Power. **Dr. L. WEBER**—Notes on Lightning Strokes in Schleswig-Holstein.

No. 7.—**Dr. DOLINAR**—See *ante*. **Prof. GOPPELSRÖDER**—See *ante*. **Dr. L. WEBER**—See *ante*.

No. 8.—**M. BURSTYN**—Firing Ships' Guns by Electricity. **Prof. SCHNEEBELLI**—Berthoud-Borel Cable in the Arlberg Tunnel. **Prof. GOPPELSRÖDER**—See *ante*. **J. WEBER**—Source of Volta Electricity.

JOURNAL

OF THE

SOCIETY OF

Telegraph-Engineers and Electricians.

Founded 1871. *Incorporated* 1883.

VOL. XIV. 1885. No. 58.

The One Hundred and Forty-sixth Ordinary General Meeting of the Society was held at the Institute of Civil Engineers, 27, Great George Street, Westminster, on Thursday, 28th May, 1885—C. E. SPAGNOLETTI, Esq., M. Inst. C.E., President, in the Chair.

The minutes of the last meeting were read by the SECRETARY, and confirmed.

The PRESIDENT announced the transfer of Mr. J. H. Rider from the class of Students to that of Associates.

Donations to the Library were announced as having been received from the following:—G. A. Rowell, R. H. Krause, Maurice Simon, and E. Hospitalier; to all of whom a hearty vote of thanks was accorded.

The following paper was then read:—

SHIP LIGHTING BY GLOW LAMPS, EMBODYING RESULTS OF TRIAL FOR ECONOMY IN H.M.S. "COLOSSUS."

By J. Farquharson, Member.

The object of this paper is to bring to the notice of the members of the Society some carefully-ascertained facts as to the relative cost of lighting ships of war by the electric and the ordinary system hitherto in use, and not for the purpose of describing any novelty or efficiency of the electric system as applied to these ships. The author has chosen the easier course of giving a narrative of ascertained and officially-recorded facts as to cost of maintenance, purposely avoiding opinions as to the merits and defects of the system adopted and tested.

H.M.S. "Colossus," 6,000 h.p., 9,150 tons.

Programme of Seven Days' Trial of Electric Lighting, to represent average requirements during the year. Commencing 7 a.m. on Thursday, the 15th, and concluding 7 a.m. on Thursday, 22nd January, 1885, the ship being lighted by day and night continuously.

Spaces Lighted, &c.

During the day from 7 a.m. to 6 p.m.

Steering engine compartment, dynamo room, engine and boiler rooms, shaft passages, engineers' workshop, engine room, artificers' wash place, stokers' and seamen's bag rooms, paint room, hydraulic pumping rooms, turret turning engine compartments, warrant officers' store rooms; also all the police lamps throughout the ship below the upper deck, and portable lamps for trimming coal bunkers.

From 6 p.m. to 9 p.m.

Officers' cabins, ward room, reading room, pantries, steering engine compartment, dynamo room, engineers' wash place, engine and boiler rooms, shaft passages, mess berths, sick bay, *main deck* aft, citadel, mess deck, lamp room, upper deck, fore

and aft under superstructure, seamen's head, officers' w.c.'s, under skid beams, conning tower, chart house; also all the police lamps throughout the ship below the upper deck, and the portable lamps for trimming coal bunkers.

From 9 *p.m. to* 11 *p.m.*

Officers' cabins, ward room, reading room, pantries, steering engine compartment, dynamo room, engineers' wash place, engine and boiler rooms, shaft passages, warrant officers' mess berth, sick bay, upper and main decks aft, under skid beams, officers' w.c.'s, seamen's head, lamp room, conning tower, and chart house; also all the police lamps throughout the ship below the upper deck, and the portable lamps for trimming coal bunkers.

From 11 *p.m. to* 7 *a.m.*

Steering engine compartment, dynamo room, engine and boiler rooms, shaft passages, conning tower, chart house, officers' w.c.'s, seamen's head, and sick bay; also all the police lamps throughout the ship below the upper deck, and portable lamps for trimming coal bunkers.

On one day, the 21st, so as to represent average time, lights were used at quarters; the following arrangement being made:—

From 10 *a.m. to* 2 *p.m.*

All police lamps and all lights in the ship, except for cabins, main deck aft and forward, store and provision rooms.

The object of the trial being to ascertain, for their Lordships' information, the cost of electric as compared with the ordinary light, for guidance as to extending its use in ships of the Royal Navy—the greater efficiency of the new light being already known—it was not considered necessary to take account of the cost of labour employed in attending the electric installation, it being understood that no increase in the ship's complement of men would be necessary, and that the cost of the artificers employed with the electric would not exceed the cost of the lamp-trimmers required for the old light. For this purpose it is only necessary to compare the *cost of the* materials consumed by the two systems

respectively, viz., the electric, as ascertained by the seven days' trial, and shown in Tables A and B, and the cost of the establishment allowance of oil and candles, based on the number of lights in the ship, as shown in the following account, for a period of seven days:—

Ordinary Light.	£	s.	d.	Electric Light.	£	s.	d.
Oil (rapeseed), 335 gallons	40	0	0	Coals, oil, and waste, for engines	9	2	4
Candles, 500 lbs.	17	0	0	Glow lamps consumed ...	11	14	6
				Oil and waste	0	10	5
				Total	21	7	3
				Balance in favour of electric	35	12	9
Total	£57	0	0		£57	0	0

It thus appears that the saving in cost of materials in the year, as per trial, is £1,852 14s. 8d. To properly estimate the financial effect of the change from the old to the new light, it is necessary to take account of the capital cost of each system, and the annual depreciation or cost of maintenance. The only point in the following account requiring special notice is, that the number of lamps is based on the assumed average life of 1,000 hours, and not on that obtained in the trial, which was abnormal and exceptional. It may here be noticed that lamps can be readily obtained with a guaranteed average life of 1,000 hours, and that the average actually obtained in the Indian troopships is more than double that number of hours. Without attempting any positive explanation of the excessive failure of lamps in the seven days' trial, the author is of opinion that it was mainly due to the high incandescence necessary to give the nominal candle-power. It will be noticed by the table that the nominal candle-power was closely adhered to throughout the trial. Such a result may raise the question how far the apparent efficiency obtained by high incandescence is really economical.

The engines were Brotherhood's three-cylinder improved simple *type*, coupled direct. The dynamos were Victoria direct-current *compound*. The lamps were Victoria; the total number, 400.

15th to 22nd, 1885.

Date.	Pressure of Steam. In Boiler.	Pressure of Steam. At Elect Engi	ned (...hyr). Per hour.	Cotton waste (colored)	REMARKS.
1885.			Cwt.	Lbs.	Coal used for lighting up = 17 cwt.
Jan. 15 ...	61	39	1·58	—	Fan engine kept running. Feed donkey as required, and fire engine running slowly for auxiliary condenser.
" 16 ...	52	43	1·92	1	Fire engine stopped at 2 p.m., and bilge donkey substituted for circulating water through auxiliary condenser.
" 17 ...	53	43	1·96	2	Fan engine and bilge donkey running throughout; feed donkey as required.
" 18 ...	52	45	2·04	1	Do. do. tubes swept soon after midnight.
" 19 ...	52	42	1·96	1	Engines as before.
" 20 ...	52	44	2·04	1	Do.
" 21 ...	52	45	1·96	1	Do. Lights as at quarters from 10 a.m. to 2 p.m.
Totals...	374	301	13·46	7	
Means...	53·4	43	1·92	1	
					with Electric Engine stopped.
Jan. 22 ...	51	—	·58	1	Fan engine and donkeys as above, and steam in pipes up to electric engines.

Started at 7 a.m. on lbs. in 7 days for 323 cwt. of coal = 3·16 per cent.
above observations being boiler.
s = 46 cwt. Coal used for maintaining steam under the same
No. 1 engine pped = 14 cwt.; leaving 32 cwt. per 24 hours as the additional
No. 2 do be required for electric engine when the boiler is already in use
No. 3 do.

	£	s.	d.
No. 1 (2nd tim cwt. per day, at 15s. per ton (Rate Book price)	8	8	0
day at 2s. 2d. per gallon	0	12	4
Reducing valve on 12s. 6d. per cwt.	0	2	0
No. 2 engine, but this wa			
very satisfactory manner Total per week	£9	2	4

Engines exhausting i

Dynamos fitted in "Colossus."

Date.	Lamps ...ken. Half.	Sperm Oil consumed.	Waste consumed.	Remarks.
15.1.85	— 5 1 0	2 pts.	½ lb.	
16.1.85	0 1 0 0 0	1½ do.	½ do.	The change of load from No. 1 to No. 2 Dynamo was made at 9 a.m.
17.1.85	3 1 1 1	2 do.	½ do.	
18.1.85	0 1 1 1 0	2 do.	½ do.	The change of load from No. 2 to No. 3 Dynamo was made at 9 a.m.
19.1.85	1 0 0 1	1½ do.	½ do.	
20.1.85	0 3 1 1 0	1½ do.	½ do.	The change of load from No. 3 to No. 1 Dynamo was made at 9 a.m.
21.1.85	1 0 3 1 0 1	½ do.	½ do.	The price of lamps £0 3s. 6d. each; Do. oil (sperm) £0 6s. 0d. per gal.; Do. waste £1 12s. 6d. cwt.
	29	12½	3½	

No...ken

	£	s.	d.
Total of lamps ...	11	14	6
Do. oil ...	0	9	4½
Do. waste ...	0	1	0½
Grand total ...	£12	4	11

ANNUAL COST OF EACH SYSTEM IN H.M.S. "COLOSSUS."

Ordinary Light.	£	s.	d.	Electric Light.	£	s.	d.
Interest on cost of plant, at 5 per cent.	31	12	0	Interest on cost of plant, at 5 per cent.	121	8	0
Depreciation, at 10 per cent.	63	4	0	Depreciation, at 10 per cent.	242	16	0
Oil and candles	2,964	0	0	Glow lamps, 285 in No.	50	0	0
				Coals, oil, and waste ...	501	3	0
				Balance in favour of electric light	2,143	9	0
Total ...	£3,058	16	0	Total ...	£3,058	16	0

The advantage of the electric, in point of efficiency and safety, in costly and complicated ships of the "Colossus" class, can hardly be estimated; but, combined with the saving in annual cost shown by this trial, the general application of electric light to all large ships of the Royal Navy may be considered as certain in future.

Being asked by the President whether he had any further remarks to add to the paper,

Mr. FARQUHARSON said: There is only one remark that I wish to make—which will be, perhaps, information that you will be glad to hear—and that is, that the figures given in the paper have not failed to produce the effect that was expected from them. Their Lordships have already given a general order that all mastless ships shall be fitted with internal electric light, and that will be done without farther question or authority. That does not mean that other ships will not be fitted. This, I think, is the only remark I have to make.

The PRESIDENT: I will now ask you to discuss Mr. Farquharson's paper. From the very satisfactory results that he has given us to-night, it is evident that electrical ship-lighting is a great success. One advantage electricity has in this application is that it has not its great opponent, gas, to contend with. No doubt for safety from fire there can be no system that can be better suited than electricity for ship-lighting.

I will ask any gentleman to kindly offer any remarks, or ask any questions he may desire, on Mr. Farquharson's paper on ship-lighting, or to give us any information he may have on this subject.

Sir David Salomons: There is one remark I should like to make on this paper, and that is not by way of criticism, but rather, I should say, as a question. I notice, on reading the column of temperatures, that the temperature of the dynamo room was something like 80 degrees. I should like to know whether this was due to the peculiar position of the room, or to the fact that the dynamos used heated the room from the coils themselves becoming hot.

Mr. J. N. Shoolbred: I notice in Table B it is stated, that no less than 62 lamps were broken out of the 400 that were on trial during the week. It must be admitted that that is a very large proportion, and I would ask whether that may not be due to the lamps being kept too closely up to the nominal candle-power? I would venture, from the result of my own experience, to say that it is. Mr. Farquharson gives no information as to what instrument he used to carry out his experiments. In my own experience I have used a number of different instruments, all supposed to be very accurate, and have found great differences in the results obtained—from 10 to 15 per cent: a difference which means a proper, or an undue use of a lamp, according to the standard by which it was originally made. The present state of our voltmeters is unsatisfactory, because one cannot be quite certain as to whether the result is correct or not. On inquiring, a short time back, as to whether there was any probability of our getting a standard voltmeter, not merely for one or two volts, but say up to 200 volts, I had the pleasure of hearing, from Sir. Wm. Thomson, that he expects very shortly to put before the public a standard voltmeter which will be able to standardise any lamps up to 200 volts. It is, I understand, a very simple instrument, and within the reach of all; as such, it will, I am sure, be welcomed by every one of us.

With regard to the cost of electric lighting on board ship, from some experience I myself have had, and from the result of careful *experiments*, where steam is used from large boilers—as I suppose

was the case in this instance—I am of opinion that, with 50 lamps burning about five hours per night, the expense of steam, &c., should be about one shilling; that is, for 200 lamp-hours. Now, supposing the average life of the lamp itself to be about 1,250 hours, one-fifth of its cost, or say one shilling more, would have to be further added for depreciation. Therefore, 250 lamp-hours would cost about two shillings, including depreciation, or about $\frac{1}{10}$ of a penny per lamp-hour. This shows how very much more economical lighting by electricity is than by oil, on board ship.

Mr. J. SWINBURNE: I should like to point out that the so-called time tests we read about really mean nothing at all. We do not know that lamps really have what is called a "life;" it is as probable that there is an efficiency under which a lamp will be practically indestructible, and above which it will soon break. If lamps have what is called a "life," to test it would require a governor to control the circuit within certainly one-half per cent., and a registering instrument to check the governor. The resistance of the lamps would also have to be taken continually. In lamp-testing the ratio of the light given to the photometer to the total light must be carefully determined; the light standard must be accurate; and if a gas photometer is used, it must be corrected for deviations from the law of inverse squares. The instruments used must be accurate, and the voltmeters and ammeters in the market cannot be taken as correct. I have tested most of these, and found only one voltmeter and ammeter within one-half per cent. There are innumerable points like these, apparently small, in connection with lamp-testing, but I have perhaps mentioned enough to show the absurdity of the so-called time tests of lamps.

Professor AYRTON: You spoke about an ammeter not being within one-half per cent. One-half per cent. of what? What was the standard?

Mr. SWINBURNE: The instruments were checked with two of Sir W. Thomson's tangent instruments in our photometer room. The tangent scales of these were checked and found correct at a convenient distance out, though the resistance of the voltmeter was wrong, *being marked* so many Rayleigh ohms, and really that

number of B.A. ohms. Probably the maker has been mixing his resistances. The voltmeter is periodically calibrated in B.A. volts from a Clark cell by the Poggendorff method, and the ammeter by deposition. A standard resistance between them was used for checking them.

Professor AYRTON: The voltmeters in the market have five distinct errors—first, the error arising from the volt standard employed by the maker being wrong; second, age error, especially noticeable in permanent magnet instruments, in consequence of which the instrument, even if graduated quite correctly when constructed, does not indicate correctly when subsequently used; third, remanent magnetism error, noticeable in any instrument which has much iron, and producing the effect that the readings for increasing currents do not agree with those for decreasing currents; fourth, extraneous disturbance error, to which all needle instruments are more or less subject; fifth, temperature error, causing a change in the resistance of the instrument, this error arising partly from changes of temperature of the room, and partly from the heat developed by the current passing through the instrument. The volt standards employed by different makers differ often as much as five per cent., and it is of the greatest practical importance that greater care should be taken to use a standard which agrees more nearly with the legal volt. The third error is also very serious, for I have tested some of the so-called "electro-magnetic" current meters in the market, and I have found that the readings in the lower part of the scale varied by over 30 per cent., depending on whether the current to be tested was increasing or diminishing. The extraneous magnetic disturbance is also very great in this type of instrument, and I know of one voltmeter which indicated a hundred volts directly the dynamo was started, and before any wires were attached to the voltmeter itself. The question of avoiding these extraneous disturbances is a subject which has interested my colleague and myself for a long period. We adopted successively various devices, such as a powerful controlling magnet, or surrounding the instrument with iron; but the best arrangement that we have found is the one employed by us two years ago in our magnifying spring instrument,

and, indeed, is, I understand, the arrangement Sir William Thomson has, among other improvements, adopted in his latest instrument, referred to this evening by Mr. Shoolbred, but not yet shown to the public. This device consists in using, not a motion of *rotation* in the thing which is moved, and the motion of which measures the E.M.F., or, it may be, the current in the case of an ammeter, but to secure a motion of *translation*. It is a most important thing, for the reason that a uniform magnetic field, no matter how strong, produces no motion of translation—only a motion of rotation—and therefore, if the moving thing has *solely* a motion of translation, we can put it as near as you like to a most powerful dynamo and practically no disturbance is produced. Of course it is well known that any small space—over the area of this sheet of paper, for example—has practically a uniform magnetic field throughout it, even although it may be within three or four feet of a powerful dynamo machine; and hence throughout that space no motion of translation is produced by that magnetic field. As a well-known example of this principle, I may remind you that the earth has no tendency to move a compass-needle bodily. Sir William Thomson has, I understand, used this principle in his new instrument by suspending a small iron ball, which is pulled down by a coil. We, on the other hand, have used a small metal cylinder, which is sucked bodily downwards into a coil. Whichever plan you employ you have a motion of translation and not of rotation, and hence you have an arrangement screened from this extraneous magnetic disturbance. I am inclined to think that no arrangement of turning on a pivot, as with a suspended needle, or, in fact, any device where there is a turning, can be successful in instruments to be used in dynamo rooms; and this I consider to be one very serious fault in almost all the instruments which are in the market. The error arising from the change of resistance of a voltmeter due to the heating caused by the current passing through it, is one that has also been engaging our attention, and we have concluded that in avoiding this error nothing is gained by winding the coil producing the magnetic deflection with German silver wire in place of copper.

Mr. Swinburne: I do not want to take up the whole evening,

but I must say that I have tested only one of Professor Ayrton's new instruments. It was too close to our instruments for our standard of volts to be five per cent. wrong. Other errors are due to temperature and to retention of magnetism with a falling current. The two I referred to as correct are not translation instruments, being Messrs. Crompton & Kapp's. They were tested with currents rising and falling without jumps, but were not near any dynamo.

Mr. Crompton: I want to call attention to a point in Mr. Farquharson's paper which, I think, ought to be noticed. I wish to explain that I have no desire to criticise the paper adversely, because I think that the result is so entirely satisfactory to us, electric-light engineers, that it would be invidious to do so. But I think, for the credit of electric-light engineering generally, we ought to call attention to the fact that, although the results in this case are good they, are not as good as can be obtained. There are certain conditions in the working of these trials which conduce to this result. For instance, if we compare the work done with the coal consumed, the result is not favourable. This is due no doubt to the very low pressure ordered to be used on Her Majesty's ships, and to other conditions we know of which militate against high economy, but which readers of the published proceedings of this Society may not be aware of. Apparently the mean horse-power throughout the twenty-four hours works out at a little over 13½ h.p. The total weight of coal divided by this figure gives 3½ cwt. per h.p., which are not good results, and is much higher than is now usual; but I think it is partially explained by the fact that the authorities insist on the use of very low pressure steam, as many of our ironclads have old and partially worn-out boilers, and these boilers are not supposed to work with more than 15 lbs. of steam. Now, I need hardly tell you that 15 lbs. of steam is not an economical pressure to work at. I do not know whether or no the pressure in this particular case was so low as this, but, at any rate, the engines used were not the engines of the present day, *i.e.*, the modern compound engine. I will show you what a difference this economy in coal would make. For instance, we

could get this coal bill down from £8 8s. per week to £3 1s., *i.e.*, £5 7s. per week. The difference is enormous. If it was not that electricity had such a very expensive rival to contend with in the shape of oil, it might easily have got beaten.

Dr. FLEMING: I notice, Sir, that in Mr. Farquharson's data which he has given us, mention is made of 62 lamps which had their fibres broken in the course of the trial run. I do not see on the paper any figures—which I should like to have seen—indicating the time they had previously been burning. Merely to say that in 188 hours——

Mr. FARQUHARSON: They had not been burned at all.

Dr. FLEMING: Out of how many were there 62 broken?

Mr. FARQUHARSON: Out of 400.

Dr. FLEMING: Because if we had such data as this, together with the more important data of the candles per horse-power at which they were burned, we should have something which could be made use of in discussing the question of their life.

Professor Ayrton has just told us that lamp makers are uncertain as to the value of electro-motive force to the extent of five per cent. I must say that I cannot agree with him there. This I do know, that there is the greatest divergency between different voltmeters professing to indicate legal volts. The correct standard of E.M.F. in which lamps ought to be marked is the legal volt; and voltmeters in use ought to be calibrated and standardised in position by the use of some known standard, such as a Clark's cell, whose value in real volts is 1·435, according to Lord Rayleigh's most recent researches. It is impossible to trust to figures on a scale. Nearly all instruments in the market are more or less incorrect, and even Sir W. Thomson's beautiful graded galvanometers require careful standardisation to correct for scale error and want of permanency in the magnet.

Mr. Shoolbred has given us some facts about the duration of Swan lamps. I do not know if he has any data as to the candles per horse-power at which they were worked. What we require to know is the watts per candle-power which is shown in practice to give a satisfactory life, and data as to life apart from other facts are not much use. The real modulus which determines the

relative value of incandescence lamps for commercial purposes is not the watts per candle-power alone, but the product of the candles' light per horse-power and the average life, divided by the price, and this numeric gives their relative usefulness.

Mr. TROTTER: One question I should like to ask. In his remarks about the lamps Dr. Fleming has raised the point, that we want to know, not only the volts and candle-power, or candles per horse-power, but the degree of incandescence. It appears that the lamps were all taken at their nominal candle-power, which of course is what the consumer wants. In no case were 81 volts reached, and the candle-power never reaches 21. It comes very near it. It would be interesting to know whether these lamps appeared to be overrun at too white a colour, in which case it is probable that the makers had an incorrect standard of volts. If the lamps were very white probably this would be the case, and perhaps some excuse for their very short life.

Mr. P. SELLON: A good deal of attention has been drawn during the evening to what at first sight appears to constitute a serious item in the results of the seven days' trial on the "Colossus," namely, the breakage of so many filaments in the incandescence lamps; and as I was present not only during the seven days' trial of which Mr. Farquharson has given us the results, but also during several previous trials made on board, I may be able to afford some explanation on this point. As the "Colossus" is the first ship lighted throughout and fitted with electrical to the exclusion of every other form of lighting, a considerable amount of experimenting of one kind and another was gone through, such, for instance, as trying the effect of steadying a hand-worked projector focussing lamp by burning a certain number of incandescence lamps in parallel circuit with it at the same time; and, further, the effect of throwing a large number of lamps suddenly off and on in order to test the self-regulating properties of the dynamos before the engine-governors were adjusted, and varying this outside load to an extent that would seldom, if ever, obtain in practical working. I say that such trials as these, though most interesting and *essential*, imposed a considerable strain upon the lamps; and the

damage done by this "over-incandescence" of the filaments naturally showed itself by the diminished life of some lamps. The efficiency of the lamps on the "Colossus," which are of 80 volts and of 20 and 10 candle-power, is slightly under 2·5 watts per candle. Experience will prove what is the most suitable efficiency to employ on board ship, and how far extra size and extra weight of engines and dynamos, and extra coal consumption, &c., involved by the use of lower efficiency lamps—say 4 to 5 watts per candle-power—will be compensated for by a longer life of the lamps; but it must not be forgotten that in the present case, and by the use of lamps supplied to work at the above-mentioned efficiency—for which a life, as mentioned by Mr. Farquharson in his paper, may be relied upon, under guarantee, of 1,000 hours, when worked under favourable conditions—a single dynamo is enabled to light the whole of the vessel sufficiently to conduct or resist a night attack, thus necessitating a second dynamo only in case the projectors on the vessel required to be in operation at the same time; whereas with lower efficiency lamps a third dynamo—acting on the "Colossus" as a spare—would require to work at the same time as No. 1 dynamo to supply sufficient light for the vessel, and thus no reserve, in case of accident to a single engine and dynamo, would be at hand.

Mr. A. Siemens: Mr. Sellon's statement that the "Colossus" was the first war-ship lighted throughout is hardly correct, as our firm had completed the lighting of the Brazilian ironclad "Riachuelo" before the installation on the "Colossus" was commenced, and the arrangements made there have been imitated to a large extent on the "Colossus." We were also fitting the torpedo ram "Polyphemus" at Portsmouth while the lights on the "Colossus" were being fitted.

The reason for the excessive breakage of lamps during the trial described by Mr. Farquharson was, as far as I know, that the lamps were of an inferior quality, and this seems to be borne out by the fact that the whole lot were afterwards removed and replaced by others. This bad lot of lamps cannot at all be taken as a criterion of the cost of lighting ships by electricity, for I am perfectly sure that if there were anything like the breakage of

sixty lamps out of four hundred per week, no lighting on ships could be carried on. As a contrast I may say that lately the s.s. "Carthage" went out to New Zealand and came back, and broke only one lamp on the trip. But then, of course, the lamps were not run up to their most brilliant candle-power, but so that they gave sufficient light for the ship's use; and that, I think, is the proper way of treating lamps—to get the light out of them which you really require, and still run them so low that their life is prolonged. I may say at once, as regards the life of lamps, we have fitted a number of lamps at the Savoy Theatre, which were put in at the beginning of 1882, and do not show any sign of going out: they are 20-candle power, run at 18 candles. The great secret of keeping lamps and improving their life is that they should be started slowly, and strictly guarded against ever getting too much current. Then the lamps will last.

Mr. F. Wyles: With regard to the question of voltmeters, a very excellent instrument is the one patented by Captain Cardew, and I think if electricians would only take the trouble to find out about this instrument, we should hear less about the trouble of getting reliable information concerning the running of the lamps. Then, again, with regard to the life of the lamps, we have some lamps at work at Waterloo Station which have certainly been running two thousand hours. Others have been put in, and they have given out in less than two hundred hours. Some of these, sent back to the Edison Company, were returned to us, and we were told that they had been run at too high E.M.F., but they had been in use under exactly the same conditions as those which had had the life of two thousand hours. I think manufacturers ought to guarantee lamps to last a thousand hours.

Mr. Sydney F. Walker: I should like to give my experience with incandescent lamps. I have done a good deal of colliery lighting, and perhaps this is as severe a test as there is for almost any lamps, and my experience tallies with almost everyone's. Lamps vary very much, and every batch of lamps sent out marked with a certain E.M.F. varies also. I recently had some 60-volt *lamps* and 65-volt lamps, and we put them on the same circuit

and could not see any difference, and the plan I have adopted invariably is the plan which Mr. Siemens suggested—to run them always under power. We invariably allow at least 5 volts, and sometimes 10 volts if we can, and I find that I get a very greatly increased life. I have had them running in a colliery where they are subjected to a very severe strain as long as three thousand hours. On the other hand, I have had lamps go at ten hours. But I think a great deal of the short life of lamps is due to mechanical injury in carriage, especially with the higher-resistance lamps; and again I find, as Mr. Siemens has suggested, that if you strain a lamp five per cent. over its normal power you have often taken out in one hour hundreds of hours of the life of the lamp.

There is a point I should like to mention with regard to instruments. I was putting up an installation the other day, and I think I had five or six instruments in the engine-house, and I found that none of them agreed. I find that the same instrument varies from time to time. Of course most of the errors have been pointed out, but there is one particular error that I have found—that is, the pivots of the needles are apt to get dirty. It is a serious error. I have had an instrument with a permanent magnet, expecting to get one result, when, owing to the pivots having rusted, we have got the opposite result, and we did not get back to the proper result again until we had had it to pieces and recalibrated it.

Mr. Farquharson: First, with regard to the temperature of the dynamo room. The dynamo room is small, and has only artificial ventilation. The whole heat of the dynamo room was due to the engine running the dynamos working in a confined place with only artificial ventilation. We cannot help ourselves in that respect. I hope we may in future, when we build ships that are to be lighted electrically; but we are obliged to do the best we can, and to keep the temperature as low as we can.

Then, with regard to the remark of Mr. Shoolbred as to the means of measuring the current, our practice is to contract for dynamos and engines to do certain work under specified conditions as to volts, ampères, steam pressure, and speed; the lamps with this current to give a specified candle-power. At a preliminary

trial, generally made at contractors' works, with a fixed external resistance approximating as nearly as can be estimated to the working conditions in the ship, the speed at which the machine does the specified work is ascertained and marked on it as its normal. In the present instance the installation had been tried and accepted as fairly satisfying the conditions of contract, and we were now trying the cost of lighting the ship under the ordinary average conditions of working, according to a programme as detailed in the paper, and previously arranged by experienced officers. The detailed particulars in tables A and B are intended to show the conditions of working under which the expense was incurred. The current and candle-power were ascertained by a Bunsen photometer and a duplicate set of measuring instruments in a room well away from the dynamos. I believe one set of measuring instruments was Crompton's, checked by Siemens' dynamometer—an instrument in which we have great faith. It was not necessary for our purpose to go into minute differences of errors in lamps and instruments such as have been discussed. That may be very interesting and very necessary for some purposes, but not for ours in the present instance.

Now, with regard to the results of this trial, I quite agree, and every one will, with the remarks made as to the advantages of running the lamps below their normal candle-power. But I should not like to put before their Lordships a statement of a trial of 20 candle-lamps run at 16 candles. If we get 20-candle lamps we try them as 20-candle lamps; and we used in this case a Bunsen photometer, taking the lamps in the best position possible, and obtained the candle-power stated in the table, our endeavours being to keep them as near as possible to the nominal candle-power.

But one object of this trial was to ascertain and report—whether favourably or unfavourably—upon the machines, the lamps, and the engines, as they are, and not as they should be. Mr. Crompton has been rather severe about the consumption of steam. Here, again, I must fall back on the same statement about ascertaining the actual state of the case. We were quite aware that engines differ very much in economy, but we had to

deal with those in the "Colossus," and not with those that may be in some other ship. But I may say, with regard to these engines, that notwithstanding the pretty large amount of steam consumed for the power given out, there are other matters which should be considered with regard to them besides economy—that is, the durability of the engines under the conditions in which we expect them to work. In this particular ship we had a simple form of engine of a well-known and reliable type. We do not propose to have that form of engine for all further use in lighting ships; but you will notice that in this case the expense of the fuel was not very high, if you consider the conditions under which we were working, and make due allowance for loss of power in the dynamo.

With regard to Mr. Trotter's question as to the whiteness of the lamps, I have stated in the paper that my own opinion is that the incandescence of the lamps was too high. There may, however, have been faulty construction besides that; I am not prepared to say that the lamps were otherwise good.

When I said that the lamps were new, of course you will understand that I mean that only the preliminary trial for taking the installation off the contractors' hands, and paying the money, had been made. We, of course, did try the light, as Mr. Sellon has stated, for a few hours' work on two or three days—that was all. So that you will understand what I mean by saying they were new lamps.

I think I have already answered Mr. Siemens' observations with regard to the lamps. I do not know whether such results as these have had any influence on the lamp makers, but I find that what used to be a 20-candle lamp is now marked 16, and we, of course, shall try these lamps as 16-candles if we have other trials like these. No doubt we shall get some advantage from it in durability.

ELECTRIC LIGHTING AT THE FORTH BRIDGE WORKS.

By JAS. N. SHOOLBRED, B.A., M. Inst. C.E., Member.

It having been decided in the autumn of 1883 by the contractors for this gigantic engineering work, Sir T. S. Tancred, Arrol, & Co., to light by electricity the large works required for its construction and erection, the author was desired by them to prepare plans, to take the necessary steps to ensure their being carried out, and to superintend the execution thereof.

The several electric lighting installations were eventually constructed, partly by Messrs. Siemens Bros. & Co., Limited, and partly by Messrs. R. E. Crompton & Co., in the proportions hereafter to be described. Before the end of 1883 the entire of the work entrusted to these two eminent firms was successfully carried out, since which time the lighting has been carried on uninterruptedly.

Having therefore stood the experience of one and a half years, including the storms of two winters, and presenting, as the installations do, many features of complete novelty, and unique in themselves, it has been deemed a subject of sufficient interest, to lay before the members of the Society of Telegraph-Engineers and Electricians a description of these electric lighting works.

It will be necessary, for the proper understanding of the subject-matter of this paper, to give a brief preliminary outline of the geographical position and character of the various parts, and of the several operations which are required to carry to a successful termination this enormous undertaking—the largest and most important engineering construction of its kind which has yet been attempted.

The delay and inconvenience, both to passengers and goods, in the railway communications along the East Coast of Scotland, occasioned by the long land detour round the Frith of Forth by Stirling, or else by the steam ferries at Granton, near the sea, and higher up at Queensferry, at length decided the North British Railway Company, in conjunction with the Great Northern and the North Eastern (users of the East Coast route from

London to Edinburgh) and the Midland (making use of the Waverley route to that city), to construct conjointly a fixed bridge across the Forth much nearer to the sea than any that exists at present.

Near to the ancient burgh of South Queensferry, about nine miles west of Edinburgh, the estuary of the Forth narrows in very considerably; while further inland it again widens out somewhat in its upper reaches. At this point, between South Queensferry on the Linlithgow shore to North Queensferry on the Fife side, the extreme breadth at high water is reduced to one mile; while midway, or rather closer to the Fife shore, a small island, called Inch Garvie, abruptly rises out of the very deep channel, some 200 feet deep, which exists on each side of it. This site, in a line nearly due north and south, seems by nature pointed out as the most suitable one. It was therefore selected in 1873 by the North British Railway Company, and a bridge was designed by the late Sir Thos. Bouch, and some of the foundations and preliminary works of which were actually begun.

After the lamentable disaster and loss of life which occurred in the overturning of the Tay Bridge in 1879, attention was drawn to the nature of the design and construction of that bridge, which, after mature consideration, it was decided to abandon.

One of the results of the lengthy inquiries held by the Board of Trade, and also before Parliamentary Committees, was to adopt the design conjointly submitted by Mr. John Fowler and Mr. B. Baker. That design may be briefly described as consisting of two huge central spans of 1,700 ft. each, on the continuous girder principle, and combining the arch, the suspension, and the lattice girder in its construction. At either extremity of this double opening is an overhanging cantilever of 681 feet. This portion, forming the central part or body of the bridge, is a few yards over one mile in length between its two extremities (called the "cantilever piers"). A viaduct of smaller spans of 168 feet each further continues the line from each cantilever pier till it reaches the solid land on either side of the estuary; the total length of the bridge being 8,091 feet, or slightly over 1½ miles.

On the south side the majority of these smaller spans are over

the water, which is here much shallower; while on the north side they are all on the land. These lengthy continuations of the main body of the bridge are in a great measure necessitated by the fact that the railway is carried at a level of 150 feet above high water.

The foundations of the piers are carried in solid masonry and concrete from depths of sometimes as much as 70 feet below high water up to a few feet above high water. Almost all the remainder of the piers and the superstructure of the bridge is constructed of steel plates (Siemens steel), in the form of flat bars and of steel tubes; some of the latter being as much as twelve feet in diameter.

About 125,000 cubic yards of masonry and 42,000 tons of steel will be used in the construction of the Forth Bridge, the cost of which will probably be about £1,800,000.

For the purposes of the construction of the bridge very large workshops have been erected at South Queensferry on the top of the Newhall Brae; and the offices, canteen, central stores, &c., are also all situated at this spot. The principal duty demanded from the electric light is the illumination of all these buildings, together with the surrounding area outside, covering a considerable amount of ground.

In order to facilitate the operations upon the piers of the smaller spans near to the Queensferry shore, a continuous wooden jetty 50 feet wide is constructed close alongside of the line, commencing close to the Newhalls pier and extending into the frith as far as the Queensferry main piers, a distance of about 700 yards.

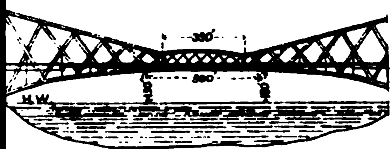
The illumination (1st) of the Newhall works and (2nd) of the Newhall staging, together with the pier foundations alongside, forms the main work for the electric light on the south side; while (3rd) on the north or Fife shore, and (4th) on the island of Inch Garvie, in mid-channel, smaller installations for the out-of-door operations, as well as the lighting of the branch stores and other buildings at each of those places, form the remaining work which is at present required of this illuminant.

The conditions to be fulfilled in the illumination of the above

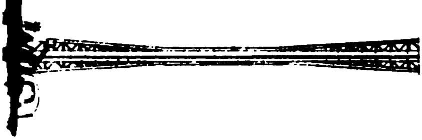
RIDGE

IGHTING.

RVIE



1700 FT



RVIE

7 Yards)

Inch.

3000

localities (owing to the ever-changing extent of the demands upon the lighting, and the variation in the exact position of each light —dependent largely upon the progress of the work in its vicinity, the state of the tide, &c., &c.) rendered it evident that the details of the lighting must be such as to allow the necessary changes to be effected readily and expeditiously, and, if needs be, even by the foremen themselves. It likewise became clear that portions of the cables, wires, and other working parts of the system, would be constantly within reach and contact of the workmen.

The primary condition (due efficiency of working being secured) seemed, therefore, to demand a system, simple in itself, which would inspire confidence among the workmen in the new illuminant, and thus gradually remove the feelings of mistrust which undoubtedly existed in their minds, owing to certain past and lamentable accidents, some of them attended with fatal consequences, in connection with electric lighting.

It became evident, also, that while arc lights would be necessary for the outside, certain of the buildings (such as the offices, stores, canteen, residential premises, &c.) required incandescent lights; and, again, that other buildings, notably the large workshops, demanded the use of both large and small lights.

Then, again, it appeared but probable that, later on, the use of storage accumulators might be desirable; and also it might be wished to devote a portion of the electric current to the transmission of power.

Thus it became further evident that the system selected for the generation of the electric current should not necessarily exclude either of these two last-named objects.

To meet these various conditions and requirements it was ultimately decided, after mature consideration, (1st) that continuous-current dynamo machines should be adopted; (2nd) that the dynamo for the outside arc lights (limited to six in series on one circuit) should not have a greater E.M.F. than 300 volts between the terminals; (3rd) that the dynamos for the incandescent lights, or for the arc and incandescent lights conjointly, should be on the compound-wound principle, with a maximum E.M.F. of 120 volts between the terminals: the incandescent

lamps to be used in parallel series with these machines to have an E.M.F. of 110 volts; while with the same machines, in lighting the large workshops, arc lamps in pairs (in series with one another) could be used on the parallel system, incandescent lamps being also placed in the circuit at the same time, as required.

In addition to the covered workshops, with their various machines, furnaces, &c., there are a number of parallel lines of rails, forming a large square uncovered workshop, and termed the "drill roads." Upon these roads circulate large movable huts, each containing circular frames fitted with an engine and drilling machines, intended for the formation of the various steel tubes, &c., of the bridge; in fact, upon these "drill roads" are first put together the various parts of the metal work of the bridge, previous to their re-erection *in situ.*

Here, as in the large covered workshops, the use of both arc and incandescent lights is necessary; but, as the exact site where each kind of light may be wanted varies with the position of the several huts, it became necessary to provide for this at certain fixed spots by supply, or "service," boxes, whence readily-fixed flexible branches lead the current to the desired site of the light.

Along the Newhall staging—projecting, as already described, into the Frith of Forth—a number of arc lights had to be provided, not merely for the illumination of the stage, but likewise to assist in getting in the foundations of the various piers occurring at intervals along its length. At the seaward extremity of this stage are the Queensferry main piers, the foundations for which consist of four cylindrical caissons, grouped in a parallelogram about 155 ft. long by 120 ft. broad (centres).

The steel caissons, 45 ft. high, have a diameter at the bottom of 70 ft., diminishing to 60 ft. at top. The bottom is left open, but about 8 ft. above it a diaphragm, or ceiling, is formed, thus converting the lower part into a sort of diving bell, wherein the workmen, descending from above through air-tight tubes, and working under atmospheric pressure, gradually excavate the stiff clay, and so allow the caisson to sink gradually down to the required depth.

Special arrangements had to be made in order to provide the

incandescent lights, fitted with strong water-tight lanterns, for the illumination. The work here continues both night and day (on one occasion as long as 840 hours without interruption). The special "portable" dynamo, compound-wound, here provides at night, in addition to the incandescent lights, some arc lights on the stage above.

This portion of the electric lighting, embracing all on the South Queensferry side, was entrusted to, and very efficiently carried out by, Messrs. Siemens Brothers & Co., Limited; while the two installations at North Queensferry and at Inch Garvie were carried out in a very satisfactory manner by Messrs. R. E. Crompton & Co.

Summary of Electrical Plant.

The above installations together contain, stated briefly—

Thirteen dynamo machines, developing about one hundred and fifty electrical horse-power.

One hundred large arc lamps (2,000 candles each).

Five hundred incandescent lamps (20 candles each).

A total length of about nine miles of mains has been laid down in the various circuits, exclusive of branch wires to lamps.

Distributing Service and Arrangements.

Main, Wires, &c.

In designing the various leads, a limit of 1,000 ampères per square inch was taken as the maximum current. This practically reduces the average working limit to about 750 ampères per square inch, or, say, one ampère per square millimètre; or, again, one and a half square inches, nearly, per 1,000 ampères—working limits which have, severally, been used successfully and recommended by distinguished authorities on the subject. The working, generally, of the mains under these conditions has proved satisfactory, no undue heating having been observed at any part.

For a considerable portion of the larger mains, naked copper wire, both stranded and also single solid, has been used, with earthenware insulators, both outside, suspended from pole to pole, and also along the interior of large sheds. The results have

been satisfactory, not merely as regards economy of first cost, but also in respect to the influence of the sea air upon these conductors (in several cases much exposed to the sea spray).

A variety of coverings—made by several firms, such as Siemens, the India-Rubber, &c., Co. (Silvertown), Henley, Callender, Phillips, &c.—offering different degrees of insulation, have been used; ranging from hemp-bound iron-sheathed cables, gutta-percha, india-rubber, &c., down to tape-bound coverings. The experience as to the lasting value of these cables and of the material of their coverings has been very varied, and sometimes anomalous. Points of imperfection, such as joints (carelessly made at points difficult of supervision), have led to a more rapid deterioration of some of these covered cables than in the case of the naked copper mains.

In designing the various mains, branches, and other wires, care was taken that their resistance should be low compared with the amount of current generally passing through them respectively. That the desired result has been arrived at is evidenced by the small loss of E.M.F. experienced by the incandescent lamps, some of which are placed at a considerable distance from the dynamo. This rate of E.M.F. is less than statements which have, comparatively recently, been made before several scientific societies would have led one to suppose.

With mains working with a current of 750 ampères per square inch of section of conductor, theory indicates a loss of 3·2 volts per 100 yards of double main; while the result of careful experiments at different parts of these works shows a much smaller rate of loss of E.M.F. For instance, at the canteen, at South Queensferry, where the greatest loss occurs (5 volts), one would expect from the distance (over 400 yards of double main) that it would have amounted to 12 volts at least. The local circumstances under which the conductors are placed have no doubt much to do with this, for the conductors are in most cases kept cool by being buried under the ground, or are exposed to the cooling influence of the sea. The behaviour of similarly designed mains, but in another installation, where the surroundings are of an opposite nature, all tending to retain the heat, would bear out the above remarks.

With respect to the different coverings of the cables and conductors, solid gutta-percha in damp, wet situations (and there are plenty of such in the above installations) has proved itself to be the best; while solid india-rubber has been equally successful where heat had to be encountered. Bare overhead copper, both stranded and solid, has stood well, so far, against the sea spray with which the atmosphere is laden at times. Gutta-percha steel-bound cable for underground, and where exposed to the wash of the sea, has also been fully successful.

Circuits for Arc Light in Series.

In consequence of the very varied calls upon these outside arc lights, which caused the particular lights to be used on any occasion to alter in position according to the requirements and progress of the work *then* going on, it became necessary to have special arrangements by which any individual arc could be kept alight or extinguished as required. It was equally necessary that this should be done without impairing the efficiency of the particular circuit in question—that is, to allow of any number of arcs, ranging from the maximum number (six) down to only one, to be burning at once on each circuit, according as the occasion demanded. Furthermore, it was required that certain circuits should have the means of being grouped together, thus to form (temporarily) but one circuit, so as to allow of the full complement (six), or less, being selected from different parts of the combined circuits. These necessities occurred chiefly at the South Queensferry works and at the Newhall staging.

These peculiar and somewhat novel conditions were met very ingeniously by Messrs. Siemens in the following manner:—The switch-board common to these arc lights in series was provided with a number of contact plugs, by means of which a variety of combinations could be effected; the result being that any particular dynamo could be connected with one or other of the circuits at will, and also that these last could be connected together as desired. Furthermore, each circuit of six lights was provided with a set of five German silver wire resistances, so that the number of lights could be altered at will from one to six, or the

entire six extinguished. These arrangements have proved of great value, and have worked well.

Circuits for Arc Lights in Parallel.

For the proper lighting of the large workshops, and also of the drill roads, both arc and incandescent lights are required. This has been done, as already stated, by the use of two large low-tension dynamos, one to each workshop, with a maximum E.M.F. of 120 volts, and therefore adapted for burning in parallel the 110-volt incandescent lamps here in use; in addition, the arc lamps, also parallel, are placed in pairs in series, each pair having its own resistance-box to enable it to burn steadily with the above E.M.F. Each dynamo can supply seven pairs of arc lamps, besides a number of incandescent lamps placed where required. Of course, the arrangement being in parallel, each bridge, whether of a pair of arcs or one incandescent lamp, can be turned on or off at will independently of the rest of the lighting by the dynamo. This, together with a number of plug-and-socket ends to the flexibles of movable incandescent portable lamps, gives great facilities for confining the illumination to any particular portion only of the workshop, and at the same time of concentrating or augmenting the amount of light at any desired point, say upon or inside of a machine or other piece of work.

Carbons for Arc Lights.

The result of a considerable amount of working, in the matter of carbons, with the above arc lights in parallel, showed that the "cored" carbons, especially those of the best quality, afforded a much steadier light for working by than did the solid carbon rods; though the hourly consumption is greater in the former than in the latter case. This experience is in accordance with the results arrived at elsewhere as to arc lights working in parallel.

A series of careful experiments upon the consumption of carbon rods with arc lights in series, and also in parallel, have been carried out here, and at other installations which are under the author's direction. Cored carbons, and also solid carbons,

from different makers, such as Siemens, Carré, Grey, Brush, Kinetic Engineering Co., &c., have been tried. Different diameters of carbons, varying from 10 millimètres to 13 millimètres, and strengths of current from 12 to 21 ampères, have been used. With respect to the cored carbons, owing to their softness, their rate of consumption is from 30 to 50 per cent. in excess of solid ones of similar diameter. With solid carbon the rate of consumption varies considerably. For instance, with a 13-millimètre rod and a 21-ampère current, it has been found to range from 1·9 to 3·0 inches per hour. Generally it may be said that for the larger diameters (within the limits above named) the rate of consumption diminishes in a degree which more than compensates for the increase in cost.

Incandescent Lamps—Duration of.

The incandescent lamps here used are of the Swan 110 volts, 20 c.p. type. During 1884 the form known as the "loop" ones were in universal use; but since the beginning of 1885 the "collar" form have been gradually replacing the former ones when requiring to be renewed.

At this, and also at other installations under the author's direction (where similar lamps are in use, under very similar conditions of working), careful records are being taken with each lamp—when put into use, its destination, the date of its ultimate fracture, and cause thereof; together with as much information as possible as to the actual time during which the lamp has been burning. As these records extend over a period of one and a half years, and embrace a large number of lamps, it may be interesting to give a summary of the average duration of these lamps. It should be mentioned that, in the case of most of the lamps, the hours of burning were long—for most of them all night through, and for many others almost continuously night and day, with but very short stoppages.

		Hours Burning.	
1st Half-Year, Jan. to June, 1884.	Average life of lamp	400	"Loop" lamps.
	Defective lamps, 35°/$_o$ (*i.e.*, under 100 hours life)		
2nd Half-Year, July to Dec., 1884.	Average life of lamp	730	
	Defective lamps, 20°/$_o$.		
3rd Half-Year, Jan. to June, 1885.	Average life of lamp	1,200	"Collar" lamps.
	Defective lamps, 10°/$_o$		

The steady improvement in the manufacture of the lamps in each half-year is very marked and satisfactory; but it is the evident superiority of the new "collar" type over the old "loop" pattern which calls for most attention.

Many of the new type of lamp have a life of 2,000 hours of burning, some as much as 3,000, and even over. The chief cause of this improvement in the lasting of the lamp is due to the fact that the platinum "loops" no longer remain exposed. In the old form of lamps want of delicacy in handling these "loops" shortened considerably, there is no doubt, the life of most of those lamps; while in the new type of "collar," as the connection with the circuit is automatically made by the operation of closing the bayonet-joint in the socket, all chances of damage to the "loops" by rough handling are avoided.

It is satisfactory to note that other makes of incandescent lamps, besides the Swan, are being manufactured with a somewhat similar form of socket and bayonet-joint; thus affording evident proof of the value of this form of connection for incandescent lamps.

The desire of the author, in the foregoing remarks, has been to present, not merely an account of the laying down of the electric installation, but also some of the experience gathered during the eighteen months it has been at work—particularly (1st) as to the distribution of the current and the loss of E.M.F. consequent thereon; (2nd) as to the relative efficiency of different types of insulating covers for wires; (3rd) as to the consumption of carbon rods for arc lighting; and (4th) as to the duration of *incandescent* lamps. The author trusts that the facts brought

forward may prove of interest to the members, and that some additional information on those points may be elicited during the discussion.

In conclusion, the author begs to offer his best thanks to Messrs. Tancred, Arrol, & Co., to Messrs. Siemens Bros. & Co., Limited, and to Messrs. R. E. Crompton & Co., for the assistance they have afforded him to enable him to bring forward the above facts; and to Mr. Sidney Baynes, the resident electrician in charge at the Forth Bridge works, for many of the particulars relative to the working of the installation, and also for the following interesting remarks.

Mr. Sidney Baynes sends the following particulars on the working arrangements for the electric light in the deep caissons at the Forth Bridge:—

"The working chamber of the caisson (about 60 ft. in diameter by 8 ft. high) is situate in the lowest part of it, and always is under considerable atmospheric pressure. Its illumination is by means of incandescent Swan lamps, 110 volts and 20 c.p. The two gutta-percha-covered mains at the surface pass into the air chamber through a stuffing-box, inside of which they are provided with ½-in. bends to prevent chafing, and which tend to guide them down the large tube provided for the descent or ascent of the men. At the bottom, in the working chamber, they are connected to a 'distributing plug' fixed on the ceiling. This 'distributing plug' consists of two solid copper rods embedded in a wooden block, parallel with each other and one inch apart. In this block there are eight square openings, each of which causes a sufficient surface of the rods to be exposed to afford a good contact. This is effected by the insertion of a wooden plug having metal springs at either side, these being connected to about six or eight yards of twin wire, which leads to an incandescent lamp. These lamps are each protected by a spherical wire-guard, and they are hung from a hook in the ceiling, wherever required. Each lamp is thus perfectly independent, and for lighting merely requires the plug end to be inserted into the distributing-plug box.

"At first, when not much work is going on, one plug-box suffices; but as the space to be lighted becomes larger, a second plug-box, with a suitable length of branch main and a wood-plug terminal, is inserted into one of the holes of the first distributing-box; and similarly with a third and a fourth distributing-box, when required.

"The entire arrangement is so simple (each part being prepared above and sent down complete) that the ordinary workmen are quite able to attend to their lamps—lighting, extinguishing, or renewing them themselves, as the case may be. A defective lamp is readily replaced by withdrawing its plug from the distributing-box, a fresh one, ready to hand, with its connections, taking its place, the defective one being at once sent above.

"For the protection of the dynamo above, a safety fuse is placed in circuit near to it, to guard against a short circuit; through branch wires being cut by the 'hydraulic spades' in use, or from any other cause. In case this fuse should melt, the man in charge of the dynamo replaces it, after a certain short fixed period of delay, the interval being utilised below to withdraw the socket-plug of the damaged wire.

"The first four of the circular deep-water caissons at South Queensferry have already been fixed under the above arrangements as regards the electric lighting. They have proved thoroughly satisfactory, chiefly through their simplicity.

"In the case, however, of the two southern deep-water caissons at Inch Garvie, owing to the very different circumstances (the bottom being rock, which had to be blasted), the illumination of the working chamber was effected by means of three arc lamps of 2,000 candles each.

"Above each lamp was a recess into which it was hoisted for protection whilst the blasting shots were being fired. Into the top of this cap, or recess, was fitted a small tube to carry off the products of combustion, which led into the air chamber, and so allowed the products to escape into the outer air.

"Two circuits, of 110 volts E.M.F., are made use of in order to guard against total extinction of the light; two of the lamps, in series, being on one circuit, and the third with a resistance (or *a fourth* arc, if necessary) on the other.

"The firing of the electric fuses in the working chamber also forms an interesting feature in these operations. From 80 to 100 shots are fired daily. These are arranged in parallels of about ten or twelve at one time. Each shot has its own pair of wires, brought from the nearest distributing-box, several of which are hung up for the purpose.

"When the shots are placed, the electricity is admitted into these mains by means of a double contact-spring, situate at the top of the ascension tube, and kept under lock and key. The ordinary dynamo supplies the necessary electricity.

"The construction of the electric fuses themselves is so simple as not to require any insulation of the wires; and although their firing is entrusted 'under sea-water,' to unskilled hands, yet the number of failures is very small indeed. These fuses are manufactured at the Forth Bridge works, at a much less cost than they could be purchased."

The PRESIDENT: I will ask you to give a vote of thanks to Mr. Farquharson for his very excellent paper, which has brought out such a capital discussion. As it is so late, I will only remark that the experience I have had in the testing of lamps quite agrees with the opinions of the various speakers. The candle-power is put too high for the volt-power, but if the lamps were 16 candle-power instead of 20 their life would be much prolonged. I would ask you to give Mr. Farquharson a hearty vote of thanks.

Mr. Shoolbred has also been kind enough to give us a capital description of the Forth Bridge, and the lighting of the works in progress. The great illumination afforded by the system of electric lighting is of great advantage in the construction of large works of this kind. It has been used by several gentlemen who have had works of this description to carry out. Mr. Walker, when making the Swansea Docks, found great assistance from it, and by its means was able to carry out his works as rapidly by night as by day. It has also been employed on the works of the Severn Tunnel, and while the Severn Bridge was being erected, and in many other places. I should like

to ask Mr. Shoolbred whether the india-rubber was plain or vulcanised which forms the insulation of the cable used at the Forth Bridge.

Professor Jamieson: There is one point which Mr. Shoolbred mentioned that I should like to draw attention to. He stated in his paper that the fall of E.M.F. between the dynamo and a certain lamp which was a quarter of a mile distant (that is, with half a mile of leading wire in circuit) was *only* 5 volts; whereas theory would indicate that the fall should have been about 12 volts, seeing that the size of copper conductor used throughout the installation was at the rate of one square inch for 750 ampères. Now, since the product of the current and the resistance gives us the fall of E.M.F. along any circuit (or $E = C \times R$), if Mr. Shoolbred had taken the resistance of the copper conductor of the circuit in question immediately before he measured the fall of E.M.F., and multiplied it by the current normally passing through it to light the lamp, he would most undoubtedly have got only 5 volts fall of E.M.F. by either method, otherwise theory must be wrong! It would be interesting to learn from Mr. Shoolbred how the cold weather could have affected the fall of E.M.F. so very much as implied by him in his paper.*

Were any of the arc lamps used under water, and, if so, what was the E.M.F. at their terminals?

---

* At the rate of 750A per square inch, Mr. Shoolbred's nearest size of conductor for a single lamp requiring ·75A would have been No. 19 B.W.G. (or ·0014 square inch cross area). Half a mile of this wire, even if of pure copper, would have a resistance of $15{\cdot}45\omega$ at 60° Fah.; and allowing that the cold weather at the Forth Bridge was such that the temperature of the wire at the time of observation was below freezing point, say at 30° Fah., the above resistance would be reduced by only ·21 per cent. per degree Fah., or to $14{\cdot}48\omega$. Therefore, since $E = C \times R = {\cdot}75A \times 14{\cdot}48\omega = 10{\cdot}86V$, fall of E.M.F. according to theory. It is most unlikely, however, that a wire of such small size as No. 19 B.W.G. was put down for such a length as half a mile to supply current even for only one lamp. A No. 16 B.W.G. wire would be nearer the usual practice; for this wire, if of 95 per cent. conductivity, would have a resistance of $6{\cdot}35\omega$ at 30° Fah. for half a mile, and consequently there would be a fall of E.M.F. $= {\cdot}75A \times 6{\cdot}35\omega = 4{\cdot}76$ volts, which agrees very closely with what Mr. Shoolbred found to be the case by his voltmeter. Theory and practice in such matters always agree, if all the circumstances are correctly noted and taken into consideration.—A. J.

Professor AYRTON: How would you find the resistance of the copper wire?

Professor JAMIESON: I would find the resistance by means of a "Wheatstone Bridge," &c., immediately before and after the lighting-up current had been applied.

SYDNEY F. WALKER: I should like to ask Mr. Shoolbred whether I understood him to say that four arc lamps from a derived circuit was the utmost number that had been worked? I have lately been working arc lamps from a derived circuit, and, as far as I can understand the compound machine, there is no difficulty in working any number up to the capacity of the machine. I thought I understood Mr. Shoolbred to say there were only four worked.

Mr. SHOOLBRED: I spoke of arcs in series. There were six in series.

Mr. F. WYLES: I would ask Mr. Shoolbred whether he had not found it necessary to use compensating resistances in the cases of arc lamps in parallel circuit?

Sir DAVID SALOMONS: A great deal has been said in the last discussion on lamps, but I have thought it best to reserve my few remarks for the present discussion. I have an installation at home which is so arranged that throughout the year I get a fairly constant E.M.F. in the mains of 98 to 102 volts, according to the number of lamps alight, and I find that the manufacture of the lamps has certainly improved. Of the first lots of a dozen at a time of "ground" lamps, delivered about two years ago, all arrived with broken filaments; these were again and again replaced, some three or four dozen having been received before twelve good lamps could be secured. As time went on, out of say 50 or 100 lamps received, this breakage diminished to 30 per cent., 15 per cent., and now all arrive in good condition. But I find the lamp-hours (or "life") for 100V lamps, 20 c.p., are only 600 on an average, although some are in good condition after a run approaching 3,000 hours.

A gentleman has referred to the Cardew voltmeter. This will, in a great measure, be the voltmeter of the future. I have some five or six voltmeters in use, and not one of them can be

altogether relied upon. Mr. Crompton was staying with me at the time when I doubted the correctness of one of the instruments. He was good enough to send for someone from his works with a standard instrument, and we found all to disagree except his own, so now I get over the difficulty by striking an average of the readings on the various instruments.

I must say it appears to me that Mr. Shoolbred must have fallen into some error in his calculation of the fall of E.M.F. in the mains, for if in the two cases given, had the mains been suited to their work, I can hardly conceive a loss of 50 per cent. more in one case than in the other, due to a difference of temperature so small as that named.

Mr. R. E. CROMPTON: Sir David Salomons has alluded to some tests of the comparative accuracy of measuring instruments made by various makers, our own included. To the best of my belief what occurred was as follows:—In the course of a visit at Sir David Salomons' house, he told me that he was surprised to find that a voltmeter recently supplied by us as a standard instrument differed very considerably from one supplied by Messrs. Siemens, also as a standard instrument. I therefore sent direct to our works for our own standard instrument, which had been carefully calibrated by Lord Rayleigh and Mr. Kapp, at Cambridge. On comparing this last-named instrument with the two abovementioned, I found that all three were practically in agreement. Messrs. Siemens' differed from ours in that theirs had been calibrated to the old volt, whereas ours was calibrated to the new volt based on the new ohm. Sir David Salomons had several other instruments by various makers, all of which were found to vary considerably from the standard.

I must say that although so much has been said and written about the inaccuracy of the voltmeter as an instrument, I cannot see that it compares unfavourably as regards percentage of variations throughout its range, with other pressure-measuring instruments, such as steam gauges, hydraulic pressure gauges, &c. I am sure that several of us makers can produce a voltmeter that we can guarantee the accuracy of to within one per cent.

I cannot let this opportunity pass without calling attention to

the many advantages presented by the voltmeter invented by Captain Cardew, particularly for engine-room use. As it is entirely independent of electro-magnetic disturbance it can be placed absolutely close to the steam engine or dynamos. As far as I have been able to judge, during the time I have been using this instrument, it does not lose its calibration to any considerable extent, and even if it does so, it can be readily re-calibrated by the use of standard instruments of the electro-magnetic class, connected parallel with it and placed at a distance from the engine-room.

The PRESIDENT: I hope that all the lamp makers and all the measuring instrument makers will profit by the discussion to-night. That there is great room for improvement is quite evident, whatever the faults may be. I will now ask Mr. Shoolbred to reply to the remarks on his paper.

Mr. SHOOLBRED, in reply, said: The first question I would advert to is referring to a remark of Mr. Siemens in this discussion, about the value of running incandescent lamps slowly, and, I would add also, turning them off slowly. My own experience of that is that such precautions add largely to the life of the lamps. Likewise, I must also concur in his remark, that when once a lamp has been strained, be it for ever so short a time, the damage done by that strain is hardly to be calculated; and it may prove very prejudicial to the life of the lamp.

The President asked, with regard to the mains which had failed, whether pure or vulcanised rubber was employed as the insulating material. I believe it was pure rubber.

As to Mr. Jamieson's remarks respecting the difference between the loss of E.M.F., as found in the observations mentioned in the paper and that which theory indicated, he himself would mention that the observations had been carried out with great care, and they had been repeated by different observers. The theoretical current-charge for the conductors, in their several parts, had been observed as far as possible, though of course this was simple enough in a laboratory, yet, in practice, and with a large network of branching conductors, it was not so easy. The chief reason, however, he considered, for the diminished loss of E.M.F. at the

Forth Bridge Works, as against that which theory indicated, was that the conditions, under which the conductors had been considered in theory (*i.e.*, in the laboratory), were very much modified in actual practice by being embedded in the soil, or carried overhead and exposed freely to the atmosphere; or again, as in the other and opposite case he had quoted, where the conductors were confined amid close and heated surroundings. These several local circumstances *must* alter the theoretical condition of the conductor and the resistance thereof, which would be modified by the varying facilities offered by the surroundings for the dissipation of its heat by radiation and convection. This influence had, moreover, been admitted by physicists. It was satisfactory to find that, in practice, sometimes, the conditions were less severe than theory imposed.

With regard to his inquiry as to the use of arc lamps under water (that is, immersed in the water itself, I presume), I do not think any have, so far, been used, except in the Inch Garvie caissons, as mentioned by Mr. Baynes in his remarks. Incandescent lamps are repeatedly taken down by divers under water, and generally protected by special cases, and they are therefore not exposed to the immediate action of the sea water; but in the early days, before the cases were fitted, the lamps themselves, in their ordinary state, were taken down attached to a gutta-percha twin wire. It was found that the action of the sea water on the platinum loops soon set up a chemical action, and they gave way very quickly. A question has been asked, whether the arc lamps in parallel had any compensation resistance. I stated in the paper that they had.

With regard to Sir David Salomons' remarks about the life of the lamp, I perfectly agree with him, and the average of 600 hours he arrived at agrees somewhat with the 730 shown in the majority of cases which came under my notice. He likewise admits that some improvement—from whatever cause—has been made in the manufacture of these lamps, as experience is gained in their manufacture. But I maintain that the great improvement is the adoption of the peculiar "collar" connection; thus doing away with the exposure of the platinum loops to rough *handling*, which always tends to destroy the life of the lamp.

Mr. Crompton's remarks with regard to voltmeters simply show the necessity of getting a uniform interpretation of the true volt by makers. I am sure that most of us will welcome a suitable standard voltmeter with great pleasure.

I have now only to thank you, gentlemen, for so kindly listening to the remarks which I have made.

The PRESIDENT moved a hearty vote of thanks to Mr. Shoolbred, which was duly accorded.

The PRESIDENT: I have great pleasure in announcing another paper on an important subject, by Mr. Snell, on "The Calculation of Mains for the Distribution of Electricity." We are very anxious to get this paper read before the autumn session, and the Council have decided to have an extra night, on the 11th of June next, for this paper.

A ballot then took place, at which the following were elected:—

Foreign Member:

Alexander Bernstein.

Associates:

T. Walter Bacon.	A. Denby Raine.
Archer Philip Crouch.	Franz Rosenader.
Edward Lowdon.	Albion T. Snell.
James F. W. Morris.	Thomas Wainwright.

Students:

Gerald Gregg.	Oswald Haes.

Leonard Newitt.

The One Hundred and Forty-seventh Ordinary General Meeting of the Society was held on Thursday, 11th June, 1885, at the Institute of Civil Engineers, 27, Great George Street, Westminster—Professor W. GRYLLS ADAMS, F.R.S. (in the unavoidable absence of the President), in the Chair.

The minutes of the last meeting were read by the SECRETARY, and confirmed.

The following transfer from the class of Students to that of Associates was announced:—

Robert Lewis, Jun.

A donation to the Library was announced as having been received from Col. Webber, R.E., C.B., Past-President. A hearty vote of thanks was accorded to the donor.

The PRESIDENT then called upon Mr. W. H. Snell, Associate, to read his paper.

ON THE CALCULATION OF MAINS FOR THE DISTRIBUTION OF ELECTRICITY.

By W. H. SNELL, Associate.

In any scheme for supplying electric light upon any large scale the determination of the proper sectional area of the conductors is one of the most important problems that we have to solve. It is well understood that, no matter what may be the particular method of distribution which has been adopted, this sectional area must in every part of the system be made to conform as closely as possible to Sir W. Thomson's law of economy; and therefore the dimensions of the conductors should be so proportioned that the annual value of the work lost in heating the metal shall equal the annual charge for interest on first cost, maintenance, and depreciation, so far as this is proportionate to their sectional area. These results may possibly require some modification when, at a later stage, we determine (1) the rise of temperature in each section under maximum current, and (2) the *distribution* of potential throughout the system. But if these

considerations demand any serious deviation from the economic standard, it will become a question whether we have selected the most efficient method of distribution.

Determination of most probable Value of "C" in the "Equation of Economy."

Now, in the equation of economy we have to make

$$P = P_1 \quad \ldots \quad \ldots \quad \ldots \quad (1)$$

where P is the annual value of the heat wasted in the conductor, and P_1 is the annual charge for the cable. P is of the form

$$P = C^2 R K \ldots \quad \ldots \quad \ldots \quad (2)$$

where R is the resistance of this section of the cable. K is some constant to be afterwards determined, and C must have a value such that a constant current equal to C would in one year produce the same total heat as does the variable and discontinuous current which is actually transmitted.* It is evident that the true value of C will always be an unknown factor, but unless we can get some approximate idea of this value the equation of economy is useless and, indeed, misleading. It is therefore important to consider by what means we may arrive at as close an approximation as the nature of the case will permit. The problem is not in itself one of great simplicity, although it permits of being reduced to a set of quadratic equations.

We see at once that the actual current will vary with the time of the day, with the day of the week, and the period of the year, as well as to a certain extent in an entirely irregular manner; and the difficulty is considerably increased by the fact that the heat wasted in the cable varies, not as the current itself, but as its square. However, it seems probable that in practice we shall usually be able to assign limiting values of C with a fair degree of certainty; and if in any case these limits are too wide to be of much service, we can then, by a sufficient amount of labour,

* The author desires to take this opportunity of recalling the fact that in the Cantor Lectures of the present year Professor George Forbes distinctly indicated the necessity of employing the average square of the current in this connection, but without going into much detail as to how this quantity may be calculated. The earlier part of the present paper was, however, written before the date of those lectures.

arrive at a still closer approximation. It must be confessed, however, that the method to be proposed for this second approximation is sufficiently clumsy and laborious, and it is very possible that something simpler or more accurate remains to be suggested.

It will be convenient to call the value of C, as above defined, "the mean annual thermal current," or, briefly, "the thermal current." In future we will write it C_θ. Put

$$\frac{C_\theta}{c} = N \quad \ldots \quad \ldots \quad \ldots \quad (3)$$

where c is the current required per lamp of normal candle power; then N is the number of lamps which, if burning continuously all the year round, would cause the same heat waste in the conductor as that actually wasted by the whole of the lamps.

Now, it is also understood that theoretically the size of the conductors should be proportionately diminished as each branch or house wire is taken off, so that the number of ampères per square inch remains constant throughout the system. But it is important to notice that this tapering must not be determined by the actual number of lamps upon the wires, but by the corresponding value of N as just defined. *It is therefore necessary to determine the value of* N *for each section of the cable.* Of course in practice there will be a limit to the frequency with which the dimensions of the conductor can be (economically) adjusted. This limit will probably vary to some extent with the size of the conductors, but for the present we shall assume that the length of each section has been provisionally determined and marked upon the plan.

We may also observe, in passing, that the rule that the number of ampères per square inch of sectional area should be constant throughout the system, is based upon the assumption that the cost of insulation and maintenance bears a constant ratio to the sectional area of the conductor. This is by no means always the case.

Now, the heat generated in any section in any period during which the number of lamps burning is constant is proportionate to the square of the number of lamps supplied through that section,

and to the length of the period. If the number of lamps alight varies from time to time, then the total heat generated during the whole period T is proportionate to

$$n_1^2 t_1 + n_2^2 t_2 + \ldots .$$

where t_1, t_2, &c., are the successive periods during which the number of lamps burning is respectively n_1, n_2, &c. Let t_1, t_2, &c., be in hours, and let T be the number of hours in one day, and put

$$n_1^2 t_1 + n_2^2 t_2 + \ldots = \mathrm{N}^2 \mathrm{T},$$

so that

$$\mathrm{N} = \sqrt{\left(\frac{n_1^2 t_1 + n_2^2 t_2 + \ldots}{\mathrm{T}}\right)} \quad \ldots \quad (4)$$

Then if for the present we assume that the respective values of n_1, n_2, &c., are constant day by day, N will have the same value as in equation (3), and will represent the number of lamps which, if burning continuously, would in twelve months generate the same waste heat as is actually generated.

Now, during the same period, if, as before, n_1, n_2, ... lamps burn for t_1, t_2, . . . hours respectively, the total number of lamp-hours per day is equal to

$$n_1 t_1 + n_2 t_2 + \ldots$$

Put this equal to M T, where M is the number of lamps which, if burning continuously, would give the same total number of lamp-hours per day.

Then we have

$$\mathrm{M} = \frac{n_1 t_1 + n_2 t_2 + \ldots}{\mathrm{T}} \quad \ldots \quad (5)$$

And if we also put $c\,\mathrm{M} = \mathrm{C}_l$, we may call C_l "the mean annual lighting current."

Comparing C_l with C_θ, or, more conveniently, M with N, we can easily see that when $n_1 = n_2 = \ldots .$, and also $(t_1 + t_2 + \ldots) = \mathrm{T}$, we shall have $\mathrm{N} = \mathrm{M}$. This is the case when the actual current is constant and continuous throughout the day; and inasmuch as the sum of the values of t can never be greater than T, it can be shown that N cannot be less than M. Therefore we have for the *minimum possible value* of N,

$$(\text{absolute min.}) \quad \mathrm{N} = \mathrm{M} \quad \ldots \quad \ldots \quad \ldots \quad (6)$$

But, as we have just seen, this can only be a real value when the values of $n_1, n_2, \ldots$ are constant and equal to each other, and the period of burning continuous.

As a matter of fact we know that the M T lamp-hours are practically performed *within* some period of (say) t hours per day. Let the average number of lamps alight at any one time during these t hours be n, then we shall have

$$n = \frac{\mathrm{M\,T}}{t} \quad \ldots \quad \ldots \quad \ldots \qquad (7)$$

Thus the *practical minimum value* of N is given by the equation

$$\mathrm{N} = \sqrt{\frac{n^2\,t}{\mathrm{T}}}, \text{ that is, from (7)}$$

$$\text{(practical min.)} \quad \mathrm{N} = \mathrm{M}\sqrt{\frac{\mathrm{T}}{t}} \quad \ldots \quad \ldots \qquad (8)$$

Next, to obtain the maximum value of N, let the total number of lamps upon the section be μ, and let τ be the average number of hours each lamp is in use per day, so that we have

$$\mathrm{M\,T} = n\,t = \mu\,\tau \quad \ldots \quad \ldots \qquad (9)$$

Then it is obvious that N will be at a maximum when μ lamps burn τ hours—that is, when

$$\mathrm{N} = \mu\sqrt{\frac{\tau}{\mathrm{T}}}, \text{ that is, from (9)}$$

$$\text{(max.)} \quad \mathrm{N} = \sqrt{\mathrm{M}\,\mu} \quad \ldots \quad \ldots \qquad (10)$$

Thus we have seen that N lies between the limits

$$\mathrm{N\,(min.)} = \mathrm{M}\sqrt{\frac{\mathrm{T}}{t}} \text{ and } \mathrm{N\,(max.)} = \sqrt{\mathrm{M}\,\mu}.$$

In (8) t really represents the total number of hours during which current is supplied, so that under Board of Trade regulations we should have $t = \mathrm{T}$ very nearly; but practically nearly all the work will be done within a very much shorter mean annual daily period—perhaps not exceeding six or seven hours—and we may with but little error reduce t in proportion. For instance, let m_1 lamps burn t_1 hours, and after this let m_2 lamps burn during t_2 hours per day, and let $m_2\,t_2$ be small compared with $m_1\,t_1$, then we may put

$$t = \frac{m_1\,t_1 + m_2\,t_2}{m_1} \quad \ldots \quad \ldots \qquad (11)$$

If we use this value of t in (8), the small error thus introduced is in excess.

In illustration of these formulæ let us suppose that on a certain cable supplying 200 lamps a run of 1,000 lamp-hours has been made within a period of 24 hours. Then

$$M = \frac{1{,}000}{24} = 42;$$

that is, the light supplied will have been equal to that of 42 lamps burning continuously throughout the whole time; and the heat wasted in the cable will have been equal to that wasted when N lamps burn continuously throughout the whole time. Then we know that N lies between the limits

$$N = M = 42; \text{ and } N = \sqrt{M\,\mu} = \sqrt{(200 \times 42)} = 91;$$

that is, the minimum value occurs when 42 lamps burn 24 hours, and the maximum when 200 lamps burn 5 hours ($\tau = 5$); the heat wasted being thus the same as if 91 lamps had burnt during the whole 24 hours. Suppose, however, that we know further that the whole of the work was performed within some period less than 8 hours, so that $t = 8$, then the minimum becomes

$$N = M\sqrt{\frac{T}{t}} = 42\sqrt{3} = 73.$$

Therefore N lies between 73 and 91.

We may suppose the actual case to have been as follows: —

200 lamps	burnt	4 hours	= 800	lamp-hours.
150 „	„	1 hour	= 150	„
50 „	„	1 „	— 50	„
	Total	...	1,000	„

$$\therefore N \sqrt{\left\{\frac{(200^2 \times 4) + (150^2) + (50^2)}{24}\right\}} = 88.$$

Thus we see that whenever we can obtain a value for M and for t we can at once assign the value of the thermal current within certain limits. And these limits become narrower as M increases towards the value of μ, and as t diminishes towards the value of τ.

Suppose, however, that these limits are found to be wider than is desirable to work from, we have a means of obtaining a closer

approximation. For this purpose we will examine another method by which we may assign a pair of limiting values for N. Let the lamps upon a given cable be divided into two groups, and assume that our information has enabled us to calculate (directly) the values of the thermal current and of the lighting currents for each group. We have thus obtained the values of N_1, N_2, and of M_1, M_2, and we have to find N and M, *i.e.*, the numbers corresponding to each current for the whole of the lamps upon the cable taken together.

It is evident that as regards M we have simply to put

$$M = M_1 + M_2 \quad \ldots \quad (12)$$

But the value of N will largely depend upon the question whether the period of running for the two groups is contemporaneous (wholly or in part) or successive. In fact N lies between the limits

$$N\,(\text{min.}) = \sqrt{(N_1^2 + N_2^2)} \text{ and } N\,(\text{max.}) = (N_1 + N_2).$$

In order to show this, in equation (4) for the value of N we will put $t_1, t_2, \ldots.$ each equal to unity. We can clearly do this with but little error by taking the unit sufficiently small, and taking as many successive values of n as are required.

We shall then have the following equations:—

$$N_1 = \sqrt{\left(\frac{n_1^2 + n_2^2 + \ldots}{T}\right)} \quad \ldots \quad (12a)$$

$$N_2 = \sqrt{\left(\frac{m_1^2 + m_2^2 + \ldots}{T}\right)} \quad \ldots \quad (12b)$$

$$\text{and } N = \sqrt{\left\{\frac{(n_1 + m_1)^2 + (n_2 + m_2)^2 + \ldots}{T}\right\}} \quad (13)$$

From (12a) and (12b) we have

$$N_1^2\,T = (n_1^2 + n_2^2 + \ldots.) \text{ and } N_2^2\,T = (m_1^2 + m_2^2 + \ldots),$$

and from (13)

$$N^2 T = \{(n_1^2 + n_2^2 + \ldots) + (m_1^2 + m_2^2 + \ldots) + 2\,(n_1 m_1 + n_2 m_2 + \ldots)\}$$

$$\therefore N = \sqrt{\left\{N_1^2 + N_2^2 + \frac{2}{T}(n_1 m_1 + n_2 m_2 + \ldots)\right\}} \quad (14)$$

In this expression for N put $\frac{2}{T}(n_1 m_1 + n_2 m_2 + \ldots) = A.$

Now when the two groups run *successively* it is clear that we shall

have $A = o$, because in each of the pairs of coefficients one of the factors will vanish. This gives for the *minimum* value of N

$$N = \sqrt{(N_1^2 + N_2^2)} \quad \ldots \quad (15)$$

But when the groups are run contemporaneously throughout A will have its maximum value, and the expression upon the right-hand side of (14) becomes a complete square. This gives for the *maximum* value of N

$$N = (N_1 + N_2) \quad \ldots \quad \ldots \quad (16)$$

We now proceed to compare these limiting values of N with those found by the former process, and to select the lower of the two maxima and the higher of the two minima to form our working limits.

The maxima are $(N_1 + N_2)$ (16) and $(\mu_1 + \mu_2)\sqrt{\frac{\tau}{T}}$, or, as it may be more conveniently written, $\sqrt{\{(\mu_1 + \mu_2)(M_1 + M_2)\}}$; these last expressions being obvious developments of (10). It is also obvious that, as the latter of these expressions represents the maximum *possible* value of N, the former cannot be greater than this. That it will usually be considerably less may be seen by taking the maximum value of $(N_1 + N_2)$, which will be $(\sqrt{M_1 \mu_1} + \sqrt{M_2 \mu_2})$, and putting this equal to the maximum possible value of N.

Then if

$$\sqrt{M_1 \mu_1} + \sqrt{M_2 \mu_2} = \sqrt{\{(\mu_1 + \mu_2)(M_1 + M_2)\}},$$

we must have $M_1 = \mu_1$ and $M_2 = \mu_2$; that is, all the lamps must burn all the day. It follows that the lower of the two maxima is given by

$$N = (N_1 + N_2).$$

Taking next the two minima, we have obtained for the general expression of the minimum value of N

$$N = M\sqrt{\frac{T}{t}} \quad \ldots \quad \ldots \quad (8)$$

and if the corresponding values of t for N_1 and N_2 be t_1 and t_2, we can write for the minimum

$$N = (M_1 + M_2)\sqrt{\frac{T}{t}} \quad \ldots \quad (17)$$

provided, however, that we have also $t_1 = t_2 = t$. For the present

we will assume that this is the case. We have for the second minimum

$$N = \sqrt{(N_1^2 + N_2^2)} \quad \dots \quad \dots \quad (15)$$

and we shall show that the former value (17) is always greater than the latter (15), except when either M_1 or $M_2 = o$, in which case they are equal.

It will be sufficient to show that the minimum value of $(M_1 + M_2)\sqrt{\frac{T}{t}}$ is greater than the maximum value of $\sqrt{(N_1^2 + N_2^2)}$. Now the latter expression is at a maximum when $N_1 = \sqrt{M_1 \mu_1}$, and also $N_2 = \sqrt{M_2 \mu_2}$. From this we get for the maximum

$$\sqrt{(N_1^2 + N_2^2)} = \sqrt{(M_1 \mu_1 + M_2 \mu_2)},$$

and this last expression is at a maximum when $M_1 = \mu_1$ and $M_2 = \mu_2$; that is finally, under these conditions, maximum value of $\sqrt{(N_1^2 + N_2^2)} = \sqrt{(M_1^2 + M_2^2)}$ ... ... (18)

Again, as t cannot be $> T$, the *least* value of $(M_1 + M_2)\sqrt{\frac{T}{t}}$ is obtained when $t = T$. Thus we have minimum value of

$$(M_1 + M_2)\sqrt{\frac{T}{t}} = (M_1 + M_2) \quad \dots \quad \dots \quad \dots \quad (19)$$

Now we have always

$$(M_1 + M_2) > \sqrt{(M_1^2 + M^2)},$$

unless either M_1 or $M_2 = o$, in which case the expressions are equal.

Thus we have proved that the higher of the two minimum values is that of equation (17).

And therefore finally N lies between the limits

$$N\ (\text{min.}) = (M_1 + M_2)\sqrt{\frac{T}{t}} \text{ and } N\ (\text{max.}) = (N_1 + N_2).$$

We have assumed hitherto (for the sake of simplicity) that $t_1 = t_2$. If this is not the case it will make no difference in the results just obtained, but we shall have to find the proper value for t in the minimum (17). For this purpose let t_2 be greater than t_1. From (7) we have

$$n = \frac{M_1 T}{t_1}; \text{ similarly put } m = \frac{M_2 T}{t_2}.$$

Then $\quad N_1 = \sqrt{\frac{n^2\, t_1}{T}}$, and $N_2 = \sqrt{\frac{m^2\, t_2}{T}}$

$$\therefore N = \sqrt{\left\{\frac{(n+m)^2\, t_1 + m^2\,(t_2 - t_1)}{T}\right\}} \ldots (19a)$$

and we have to find a value for t such that N is also equal to

$$\sqrt{\left\{\frac{(n+m)^2\, t}{T}\right\}} = (n + m)\sqrt{\frac{t}{T}} = (M_1 + M_2)\sqrt{\frac{T}{t}}.$$

Equating (19a) with the second of these expressions it will be found that (when $t_1 < t_2$)

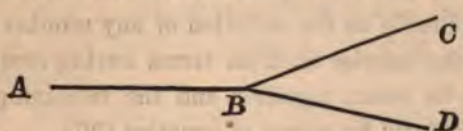
$$t = t_1 + \frac{m^2}{(n+m)^2}\,(t_2 - t_1) \ \ldots \qquad (20)$$

From this it will be seen that if $m = n$

$$t = \frac{3\, t_1 + t_2}{4} \qquad \ldots \quad \ldots \qquad (21)$$

If m is large compared to n, we have $t = t_2$ nearly. If n is large compared to m, we have $t_1 = t$ nearly.

Let us now suppose a case in illustration of these formulæ. Let there be a feeder A B, which splits into two branches B C and



B D. The data for the two branches being given, it is required to find the thermal current in the feeder—or the number of lamps to which it corresponds. We have given

(for B C)	$\mu_1 = 400$	(for B D)	$\mu_2 = 250$
	$M_1 = 60$		$M_2 = 40$
	$t_1 = 5$		$t_2 = 6$

We must suppose that N_1 and N_2 have also been independently calculated (by means of equation (4) or certain developments of that equation, which we shall presently consider); but in order that we may not assign for them impossible values, we will find the limits of N_1 and N_2 by means of equations (8) and (10).

N_1 lies between $\left\{\sqrt{M_1\,\mu_1} = 155\right\}$ and $\left\{M_1\sqrt{\frac{T}{t_1}} = 131\right\}$

N_2 ,, ,, $\left\{\sqrt{M_1\,\mu_1} = 100\right\}$,, $\left\{M_2\sqrt{\frac{T}{t_2}} = 80\right\}$

Suppose we put $N_1 = 140$ and $N_2 = 90$, assuming, as we have already said, that these particular values have been obtained by an independent method of calculation, then we know that

$$N \text{ lies between } (M_1 + M_2)\sqrt{\frac{T}{t}} \text{ and } (N_1 + N_2).$$

We may, if we like, obtain the exact value of t by means of (20), or, using (21), we can see that $t = 5{\cdot}25$ nearly, say $t = 5{\cdot}2$. We shall now obtain

$$(M_1 + M_2)\sqrt{\frac{T}{t}} = 215, \text{ and } (N_1 + N_2) = 230;$$

so that N lies between 215 and 230.

It will be interesting to find the numerical values of the other limits. We shall find that

$$\sqrt{N_1^2 + N_2^2} = 167, \text{ and } \sqrt{\{(\mu_1 + \mu_2)(M_1 + M_2)\}} = 255.$$

The reason why the three superior values happen to be comparatively near together is that the values of t_1 and t_2 were taken rather near τ_1 and τ_2.

It is evident that as regards the maximum limit of N the formula is applicable to the collation of any number of terms at once, and for the inferior limit all terms having nearly the same values of t can be taken together, and the remaining terms can be taken successively by means of equation (20).

Having arrived at this point we can see that if the lamps upon any given section are classed in any number of divisions, and if for each division we can calculate the values of M and N with any probable degree of accuracy, we shall afterwards be able to collate these divisions and find the limiting values of the thermal current for the whole section. But, further, our knowledge of the method according to which the lamps have been classed in the several divisions will in each case suggest a means of arriving, with more or less accuracy, at the value of A in equation (14). We shall thus get still nearer to the most probable value of N for the whole section.

After this we shall have to collate the thermal currents of the various sections with each other, working up towards the machines, and here we shall have to rely almost entirely upon the method *of limits.* It should, however, be noted that at any point in the

process the ratio between the superior and inferior limits of the final thermal current for any given section will be *less* than the (arithmetical) mean value of the ratios between the limiting values of N_1, N_2, &c., from which it is obtained, except in the case when all these ratios are equal. For example, taking the first section—and therefore the largest conductor—although the *difference* between the superior and inferior limits of the possible values of N is here at a maximum, yet the *ratio* between these two values cannot in any case be greater than the mean values of the ratios of the values of N_1, N_2, ... for all the other sections.

We have yet another step which it may sometimes be worth while to take. At any point in the process of collating the values for the separate sections we can make a direct calculation of the value of the thermal current for all the lamps which are supplied through that section, by proceeding as though all lamps lying beyond were connected directly to that section, finding the thermal current for the different groups, and collating as before. But although this process may be valuable as a check, especially in the case of some of the larger mains, it will be seen in the sequel that in most cases it involves very long and somewhat complex calculations.

One other point may be mentioned here: we have been obliged always to assume that the current supplied to the lamps connected directly to any given section flowed through its whole length. Virtually we treat the case in which all the lamps are placed at the extreme end of the section to which they are connected. The error thus introduced is of course one of excess; it may be minimised by judiciously placing the sections, and also by increasing their number. It may sometimes be desirable for this and other reasons to make the calculations for a much larger number of sections than we shall probably actually employ.

We have now to consider in what way the principles which we have sought to establish may best be applied. The following scheme is an attempt to determine the most probable value of the thermal currents as the problem will usually present itself in practice. We need only describe in detail the method employed to find N; this will involve all the data for M (so far as these quantities are capable of *being ascertained*).

Now the whole of the lamps supplied from any lighting station may obviously be placed in one of two divisions, according to whether their period of burning is (1) regular or (2) irregular.

Among the "Regular" lamps we shall include all those whose usage is in any sense periodic, whether or not they are in daily use. This division may again be separated into two main groups—

Group I.: Containing all lamps which are lit *at dusk* and extinguished either at dawn or at any hour which can be approximately assigned.

Group II.: Containing lamps which, although in periodic use, are not necessarily lit at the hour of dusk.

We shall find it necessary later to further subdivide each of these groups, but for the present we may leave them intact. It will be evident that Group I. includes all street lamps, very nearly all those in shops, warehouses, offices, clubs, railway stations, &c., &c., and probably a very considerable proportion of those in private houses. Group II. consists principally of those in places of worship, halls, theatres, and similar public buildings. The "Irregular" division contains all remaining lamps (apparently *chiefly* in dwelling-houses) for which it is not possible to assign any approximately periodic or constant usage. It is evident that even if in any case the "Irregular" lamps form a numerically large class, yet by far the larger share of the total work will be performed by the 1st division. It is therefore worth while to work out the probable thermal current for this division with as much accuracy as possible.

Division I.—"Regular" Lamps.

We have first to decide how we shall take account of the variation in the number of lamps alight due to the time of the day, the day of the week, and the period of the year. Now we know that the total annual period from sunrise to sunset is the same on all parts of the surface of the earth, except where modified by local physiographical or topographical features. It follows that the (normal) mean annual night is everywhere twelve hours long, and we may take it to last from 6 p.m. to 6 a.m. (local time).

Group I.

It follows that for Group I., in which the lamps are lit at dusk, we shall be able, *for the purpose of this particular calculation*, to consider the hour of lighting to be 6 p.m. all the year round. This will give us the same number of lamp-hours (whatever may be the hour assigned for their extinction) as if we reckoned from the true hour of sunset day by day. This statement, however, requires one obvious qualification: clearly, if there are any lamps which in winter-time are lit at dusk and are extinguished before 6 p.m., they will be excluded from this reckoning. We shall therefore have to form a sub-group (which we will distinguish as Group IA.) for these lamps; and, when necessary, we must make a separate calculation for them by a method which will be afterwards dealt with.

Having thus determined the normal hour of lighting for these lamps, we shall have to ascertain the usual hour of extinction in each case from the individual consumers. This ought not to be a matter of much difficulty. In order that we may see clearly the nature and extent of the information required, it may be worth while to sketch out a form of circular which might be issued for this purpose. Omitting the necessary preamble, we might proceed to ask the following questions *:—

(1) What is the total number of lamps required on your premises?

(2) How many of these are lit, as a general rule, at dusk?

(3) At what time (approximately) are they usually extinguished?

(*Specimen answer*) 6 at 8.30 p.m., 4 at 10 p.m., 2 at 11.30 p.m.

(4) If answers (2) and (3) do not apply to each day of the week, please state the exceptions.

(*Specimen answer*) On Saturdays 10 lamps burn until 10.30 p.m., and the other 2 until 12 p.m. On Sundays 6 lamps only burn until about 11.30 p.m.

* It is obviously desirable that this information should be obtained in the month of December or of January.

(5) With regard to those lamps, if any, which you do not usually light at dusk, any approximate information as to their use will be esteemed.

Having obtained this information and summarised it (for each section of the cable separately), we shall be in a position to calculate the probable value of the thermal current. From what has been said before, it will be understood that any lamps burning till dawn are reckoned as being extinguished at 6 a.m.

In the first instance we have equation (4)—

$$N = \sqrt{\left\{\frac{n_1^2 t_1 + n_2^2 t_2 + \ldots}{T}\right\}}$$

It will probably be deemed sufficiently accurate (at least for our present purpose of illustration) if we make the periods $t_1, t_2, \ldots$ each equal to one half-hour. The formula then becomes

$$N = \sqrt{\left\{\frac{n_1^2 + n_2^2 + \ldots}{2\,T}\right\}} \quad \ldots \quad (21a)$$

where n_1 is the total number of lamps (in this group) burning during the interval from 6 p.m. to 6.30 p.m., n_2 is the number burning from 6.30 p.m. to 7 p.m., and so on. But as it stands at present this formula takes no account of variations due to the day of the week. We will suppose that the group presents a well-defined variation in the numbers both for Saturday and for Sunday. We shall now have to make, in effect, three sub-divisions of Group I. And although it is true that we shall here count many, or perhaps all, of the lamps over again in each sub-division, yet it will be obvious that this is equivalent to the case in which we have two or more distinct sets of lamps *which are never run contemporaneously.*

We will indicate the values of N in Group I. for week-days by N_1, for Saturdays only by N_1', and for Sundays only by N_1''.

We shall then have for N_1 (five days per week),

$$N_1 = \sqrt{\left\{\frac{5\,(n_1^2 + n_2^2 + \ldots)}{7 \times 2\,T}\right\}} \ldots \quad (22)$$

But it will generally be more convenient to make the period annual instead of weekly, as we can then readily take account of the six annual holidays, or of any similar variation. Putting

$(365{\cdot}25\ \mathrm{T}) = \mathrm{Q} = 8{,}766$, the number of hours in one year, we shall have

$$\mathrm{N}_1 = \sqrt{\left\{\frac{d_1\,(n_1^2 + n_2^2 + \ldots)}{2\,\mathrm{Q}}\right\}} \ldots \quad (23)$$

and, similarly,

$$\mathrm{N}_1' = \sqrt{\left\{\frac{d_1'\,(n_1^2 + n_2^2 + \ldots)}{2\,\mathrm{Q}}\right\}} \ldots \quad (24)$$

$$\mathrm{N}_1'' = \sqrt{\left\{\frac{d_1''\,(n_1^2 + n_2^2 + \ldots)}{2\,\mathrm{Q}}\right\}} \ldots \quad (25)$$

in which d_1, d_1', and d_1'' represent respectively the number of days *per annum* for which each sub-division is to be counted. The values of $n_1, n_2, \ldots$ of course will differ in the three formulæ, but it has not been thought necessary to employ any distinguishing mark.

In order to obtain the values of the numerators it is convenient to prepare a table in which to set out the values of $n_1, n_2, \ldots$ and their squares. A separate table will of course be required for each sub-division.

The following is given as a specimen. To save space, it is supposed that all the lamps are extinguished by 9 p.m. save some which burn till dawn.

SPECIMEN TABLE.

"Regular" Division—Group 1.

(Cable x, Section y) N_1 (5 days per week) $d_1 = 255$, $\mu_1 = 360$.

A.	B.	C.	D.	E.
	p.m.			
1	6.30	40	360	129,600
2	7. 0	60	320	102,400
3	7.30	0	260	67,600
4	8. 0	20	260	67,600
5	8.30	100	240	57,600
6	9. 0	40	140	19,600
...	...	0	100	...
...	...	0	...	...
	a.m.			
24	6. 0	100	100	180,000
		Total	... ...	624,400

Column A gives the subscripts of n.

„ B gives the time of expiration of the corresponding half-hour.

„ C is obtained from the summary relating to this particular section, prepared from the replies to the circular, and gives the number of lamps *extinguished* at the end of each half-hour.

„ D gives the number of lamps *left burning* during each such period.

„ E gives the squares of the preceding column.

μ_1 is here the number of lamps in the group, not the total number on the section (which we write μ). It is therefore evident that the first number in column D will always be μ_1, and the other numbers are obtained by successively subtracting the numbers in column C. In this case by means of formula (23) we shall obtain $N_1 = 93$.

It may conduce to clearness if we here re-state what is implied by this result. We find, then, that if upon this section 93 lamps burn night and day all the year round, the same amount of energy would be wasted in the conductor as is actually wasted by the 360 lamps which each burn so many hours in the day on 255 days per annum *only*. d_1 was put equal to 255 by omitting Saturdays, Sundays, and six holidays.

The values of N_1' and N_1'' having been found in the same way, we proceed, before collating these three terms, to complete the calculation of the other groups for the same section.

Group II.

It will be convenient to take next Group II., omitting Group IA. for the present. We shall probably be able to assign the mean annual usage per day for the lamps in Group II. with a fair degree of accuracy in most cases. These lamps, being chiefly in public buildings, will sometimes be lit at dusk, sometimes later, and in some cases earlier, according to the character and use of the building. But there will rarely be more than one such installation upon any one section of the cable, simply because, *after* supplying such an installation, we should almost invariably

reduce the size of the conductor. We shall therefore content ourselves in this place with a formula adapted for finding the thermal current for one such building only.

Now, in this case the number of lamps in use may be assumed to be constant throughout the (daily) period of lighting (if this is not so, at least the great majority of the lamps will be "on" or "off" together). We shall require two sub-divisions as a general rule. For week-days we will write N_3, and for Sundays N_3''. To find N_3,

Let d_3 be the number of week-days per annum for which the lamps will be used.

Let the average period during which the *building* (not the lamps) is in use on each of these days be from the time a to the time ω.

Let t' be the mean annual number of hours per day from dusk up to the time ω;

and t'' the same number up to the time a.

The values of t' and t'' are given in Table I., column C.

Then we shall have

$$N_3 = \sqrt{\left\{\frac{d_3\, n^2\,(t'-t'')}{Q}\right\}} \qquad \ldots \qquad (26)$$

TABLE I.

A.	B.	C.	A.	B.	C.
p.m.			p.m.		
4.30	36	0·098	11. 0	1787·5	4·9
5. 0	91	·249	11.30	1970	5·4
5.30	151·5	·415	12. 0	2152·5	5·9
6. 0	229	·627	a.m.		
6.30	323	·9	12.30	2335	6·4
7. 0	433	1·186	1. 0	2517·5	6·9
7.30	561	1·537	2. 0	2882·5	7·9
8. 0	704	1·93	3. 0	3247·5	8·9
8.30	875	2·4	4. 0	3603	9·87
9. 0	1057·5	2·9	5. 0	3875·5	10·617
9.30	1240	3·4	6. 0	4080·5	11·18
10. 0	1422·5	3·9	7. 0	4223·0	11·57
10.30	1605	4·4	8. 0	4300·5	11·78

Column A gives the successive half-hours from 4.30 p.m. to 8 a.m. (here we take a and ω).

" B gives the total annual period of darkness from sunset to each half-hour.

" C gives the values of (B ÷ 365). t' and t'' are taken from this column.

Whenever a has any constant value earlier than 4.30 p.m. we shall have $t''=o$. (This table is only correct for the latitude of Greenwich.)

As an illustration suppose that a building lit by 100 lamps is used two nights a week all the year round from 7 p.m. to 8.30 p.m., we shall have $n = 100$, $d = 104$, $t' = 2{\cdot}4$, $t'' = 1{\cdot}186$.

$$N_3 = \sqrt{\left\{\frac{104 \times 100^2 \times 1{\cdot}214}{8{,}766}\right\}} = \sqrt{144}$$

$$\therefore N_3 = 12.$$

Group IA.

Take next Group IA. This group accounts for lamps which properly belong to Group I., but whose normal hour of extinction is at 6 p.m. or earlier. Let the required thermal current correspond to N_2 lamps; then, if we reckon as before by half-hours, there will be four terms from the earliest period of dusk (in these latitudes) to 6 p.m. The value of N_2 is therefore given by

$$N_2 = \sqrt{\left\{\frac{d_2\,(n_1^2\,t_1 + n_2^2\,t_2 + n_3^2\,t_3 + n_4^2\,t_4)}{Q}\right\}},$$

where $t_1, t_2, \ldots$ are the first numbers in column C, Table I. It will perhaps require a moment's consideration to see that if these lamps are in use on *every* day of the year on which there is darkness before 6 p.m. we must put $d_2 = 365$; if they are not used on Sundays, $d_2 = (365-52)$, and so on.

It is obvious that this method of calculation might be adopted for all the "Regular" lamps, but it will be seen that it would usually involve considerably more labour in working out the results, as the coefficients of the squares of n would no longer be unity, and their proper values would also vary with the latitude.

"Irregular" Division.

For the "Irregular" division all that can be done is to take

the best available estimate of the number of lamp-hours they may be expected to make per annum, and thence obtain a value of N_4 by means of equations (8) and (10). It seems probable that, except perhaps in residential districts, N_4 will seldom be large in comparison with the other terms of N.

Daylight Work.

It may sometimes be desirable to form another value of N, to account for the whole of the work performed during the hours of daylight. We will call this N_5, but we need not stay to consider how it should be formed.

Collation of Groups.

We have now to collate these separate terms so as to find the value of the thermal current for all the lamps upon the section. For the sake of clearness at a subsequent stage we shall write this collated value S_a. $S_a, S_b, \ldots$ will therefore be the number of lamps corresponding to that part of the final thermal current in each section *which is proper to the lamps supplied directly from that section.*

Definition of "final thermal current."

We reserve the term "*final* thermal current" to indicate the ultimate value of C_θ, which includes the current transmitted through the particular section to sections beyond. We will afterwards show more in detail how this may be estimated in a system of conductors. The number of lamps corresponding to this final thermal current will be written $\phi_1, \phi_2, \ldots$

We will here collect the various terms of N for convenience of reference:—

Division (1)—"Regular" lamps—

Group I.—Lamps lit at dusk		... ...	N_1
„ I′.	„	for Saturday work only ...	N_1'
„ I″.	„	for Sunday work only ...	N_1''
„ IA.	„	not in use after 6 p.m.	N_2
„ II.	„	in public buildings (chiefly)	N_3
„ II″.	„	„ for Sunday work only	N_3''

Division (2)—"Irregular" lamps ... ... N_4

„ "Daylight" work ... ... N_5

This list has a somewhat inconsequent air, but after what has been already said it will not be necessary to add any further explanation of the *raison d'être* for this classification. It is obvious that the scheme is very elastic, and that, as we have attempted to deal with the most general case, it may often be simplified in actual use.

To find S_a it will be necessary to deal with these terms successively, for it is obvious that they do not all require the same method of collation. It will be seen that the following is probably the best order:—

(1)	Combine N_1	with N_1';	call the result	N_a
(2)	„ N_a	„ N_2	„	N_b
(3)	„ N_b	„ N_3	„	N_c
(4)	„ N_1''	„ N_3''	„	N_d
(5)	„ N_c	„ N_d	„	N_e
(6)	„ N_e	„ N_4	„	N_f
(7)	„ N_f	„ N_5	„	S_a

We have for the fundamental formula (*vide* equation 14 *passim*)—

$$N_{(x+y)} = \sqrt{(N_x^2 + N_y^2 + A)} \quad \dots \quad (14)$$

$$\text{where } A = \frac{2}{T}(n_1 m_1 + n_2 m_2 + \dots .).$$

The accuracy of our results in each case will now depend, as far as this process is concerned, upon the accuracy with which we can find the value of A. In some cases, as in the first, this can be done very completely. We will now go through these cases in succession.

(1) *N_1 with N_1'.*—These groups can never run contemporaneously. Therefore we have $A = o$ (see page 404), and the formula reduces to

$$N_a = \sqrt{(N_1^2 + N_1^{2\prime})} \quad \dots \quad (29)$$

(2) *N_a with N_2.*—By means of Table I., find the total annual number of lamp-hours for this group (IA.), and put this equal to $m\,q$, where q is the total number of hours of running per annum. The value of q may be obtained from column B; if the lamps are *not in use* on every day of the week, it must be multiplied by the

proper fraction ($\frac{5}{7}$ or $\frac{4}{7}$, &c.). Now, during the whole annual period of q hours during which Group IA. is at work, the number of lamps burning in Group I. remains constant at its first value, equal to n_1. If we assume (as will most usually be the case) that no work is performed by Group II. during this period, we shall have

$$N_b = \sqrt{\left(N_a^2 + N_2^2 + \frac{2\, n_1\, m\, q}{Q}\right)} \quad \ldots \qquad (30)$$

(3) *N_b with N_3.*—This case is a little more complex. Assume, first, that the section has only one installation belonging to Group II. As before, let a and ω represent the respective times of use and disuse of the building; and let t' be the mean annual number of hours per day from dusk up to time ω, and t'' the same up to time a. Also put

$$p = \frac{n_a + \ldots . n_\omega}{\omega - a + 1}.$$

So that p is the arithmetical mean of the successive values of n (for Group I.) during the time $(\omega - a)$: these terms of n will be taken from the table formed for Group I.

Then we shall have

$$A = \frac{2\,(t' - t'')\, d_3\, m\, p}{Q}.$$

If a = 8 p.m., or any later hour, we shall get

$$(t' - t'') = (\omega - a).$$

This is also the case when the lamps are always lit at the given hour, a, whether there is still daylight or not.

Thus, for one installation in Group II.,

$$N_c = \sqrt{\left\{N_b^2 + N_3^2 + \frac{2\,(t' - t'')\, d_3\, m\, p}{Q}\right\}} \quad \ldots \quad (31)$$

Take, now, the case in which there is more than one such installation upon the section.

Let $k_1, k_2, \ldots .$ be the actual numbers of lamps in each, and let $K_1, K_2 \ldots .$ be the numbers corresponding to the thermal currents which have been found for each respectively. If, as might possibly happen, these installations are regularly used concurrently, or very nearly so, we should merely have to put

$$N_3 = K_1 + K_2 + \ldots$$

and
$$m = k_1 + k_2 + \ldots$$

and to insert these values in (31). In the more general case the expression for A will have the form—

$$A = \frac{2}{Q}\Big(d_1 t_1 k_1 p_1 + d_2 t_2 k_2 p_2 + \ldots \delta_1 t_{01} k_1 k_2 + \ldots . \\ \Delta_1 t_{001} k_1 k_2 k_3 + \ldots . \Big) \quad \ldots \quad \ldots \quad (32)$$

where $d_1, d_2, \ldots .$ is the total number of days of running for each installation per annum; $\delta_1, \delta_2, \ldots .$ is the number of days on which each (possible) pair of installations run together; $\Delta_1, \Delta_2, \ldots .$ is the number of days on which each set of three installations run together; and so on. Also $t_1, t_2, \ldots .$ are the values of $(t' - t'')$ for each installation; $t_{01}, t_{02}, \ldots .$ are the values of $(t^{\text{III.}} - t^{\text{IV.}})$ for each pair of installations, where $t^{\text{IV.}}$ is found in Table I. for the time when the two installations *begin* to be running together (that is, the time corresponding to a for the later of the two), and $t^{\text{III.}}$ is found for the earlier of the two values of ω. $t_{001}, t_{002}, \ldots .$ are similar values found in the same way for every three installations; and so on.

By a similar development of equation (26) we should get for the value of N_3 an expression of the form

$$N_3 = \sqrt{\Big[\frac{1}{Q}\Big\{d_1 t_1 k_1^2 + \ldots . \delta_1 t_{01} (k_1 + k_2)^2 + \ldots . \Big\}\Big]} \quad (33)$$

(4) *N_1'' with N_2''.*—This case is the same as the last, being the Sunday work for the same groups.

(5) *N_c with N_d.*—This case is similar to case (1), collating the total Sunday work with the week-day work.

(6) *N_e with N_4.*—This case deals with the "Irregular" lamps; it is therefore impossible to obtain any definite value for A, and we can only make a choice for N_f between the limiting values (page 406.)

(7) *N_f with N_5.*—N_5 being the "daylight work" the periods are exclusive, and we have

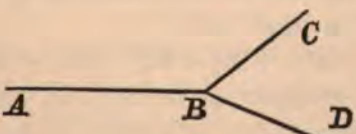
$$S_a = \sqrt{(N_f^2 + N_5^2)}.$$

Having thus obtained the values of the thermal current proper *to each* separate section in the system, we have now to find the

"final thermal current" when the section is considered *in situ* and (unless a terminal section) transmits current to sections beyond. In the case when any given section transmits *the whole* of the current for one or any number of sections beyond, there is no difficulty in determining at least the limiting values of the final thermal current by the formulæ already considered. Now, in his recent Cantor Lectures, Professor Forbes has shown that the above condition is a characteristic of *any* system in which the highest economy is attained. Referring to the "network" system, where the current has more than one path to a given lamp, he says "we should [in this case] have to choose our conductors exactly of the same size as we have chosen them here, and should have exactly the same conditions. If our conductors were chosen of the proper size for the current flowing, then the current would in that case [the 'network'] flow along exactly the same channels as under the system I have been describing [the 'multiple tree' system]. That would be the case when we had the maximum number of lamps on."

In this last sentence, if the size of the conductor is based upon the value of the thermal current, we may, I think, venture to insert the expression $\phi \sqrt{\frac{T}{t}}$ in place of the "maximum number;" $\phi \sqrt{\frac{T}{t}}$ being the number of lamps corresponding to the final thermal current when the period is reduced from T to t hours; t being the mean daily period during which the major part of the work is done, as in (8) and (11). Thus this expression is very nearly the mean number of lamps which burn together.

It will therefore be sufficient to consider as a type the case in which two terminal sections, B C and B D, unite with the section A B.



Let the "proper thermal current" for B C correspond to S_1

lamps, that for B D to S_2 lamps, and for A B to S_3 lamps, then we shall have for the numbers corresponding to the final thermal currents,

$$\phi_1 = S_1; \qquad \phi_2 = S_2$$

$$\phi_3 = \sqrt{(S_3^2 + x^2 + B)},$$

where

$$x = \sqrt{(\phi_1^2 + \phi_2^2 + A)};$$

so that

$$\phi_3 = \sqrt{(S_3^2 + \phi_1^2 + \phi_2^2 + A + B)}.$$

And if the numbers of lamps alight in each half-hour (from 6 p.m.) be for B C, $p_1, p_2, \ldots$; for B D, $q_1, q_2 \ldots$; and for A B, $u_1, u_2, \ldots$, we shall have

$$A = \frac{1}{T}\{p_1 q_1 + p_2 q_2 + \ldots\}$$

(the coefficient reduces to $\frac{1}{T}$ because the values of p and q are for half-hours—see page 404); and also

$$B = \frac{1}{T}\{u_1(p_1 + q_1) + u_2(p_2 + q_2) + \ldots\}$$

Now, as long as we have only or mainly to deal with lamps belonging to Group I. ("Regular" division), we can obtain nearly the true values of A and B without much trouble. Afterwards we may, if we please, continue to work out these values by means of formulæ similar to those already discussed in collating the groups; or we may content ourselves with assigning the limiting values of ϕ_x.

These values are, for the maximum,

$$\phi_x = S_x + \phi_y + \phi_z \quad \ldots \quad \ldots \quad (37)$$

and for the minimum,

$$\phi_x = \left(V_x + V_y^1 + V_z^1\right)\sqrt{\frac{T}{t}} \quad \ldots \quad (38)$$

where V is equal to the sum of the values of M for the different groups of the given section; M, as before, being the number of lamps corresponding to the mean lighting current for the several groups. Indicating the different terms of M similarly to those of N on page 417, we shall have

$$V = (M_1 + M'_1 + M_1'' + M_2 + M_3'' + M_3 + M_4 + M_5) \quad (39)$$

The values of V corresponding to the *final* lighting current for the section are distinguished by a dash. Thus

$$V_x^1 = V_x + V_y^1 + V_z^1 \quad \ldots \quad \ldots \quad (40)$$

When working in this way it will usually be desirable to

retain the two limiting values of ϕ all through until the calculations are complete. We shall then be better able to decide how many and what different sizes of conductors should be actually laid down. In finally choosing a value between the limits, it must be remembered that we have uniformly assumed that the lamps are placed at the extremity of each section, and that we have thus introduced an error in excess. Other errors contained in the formulæ are (probably) mainly due to the exclusion of the "Irregular" lamps in certain places, and are thus errors of deficiency.

It will be obvious that any value of ϕ may be directly determined by treating the given section as though it was terminal, distributing all lamps supplied through it into their several groups, finding values for N and collating them as before. This process would usually involve much extra trouble, especially if there are many installations belonging to Group II. (see page 414). But for determining the dimensions of "feeders," and similar sections of any considerable length, it would probably be worth while to undertake the work.

Equation of Economy.

Returning now to the "equation of economy," it may conveniently be put into the form—

$$W = C_\theta \sqrt{\rho\, b} \quad \dots \quad \dots \quad (41)$$

where C_θ is the final thermal current for the given section, W is the weight of the required conductor in pounds per unit length, ρ is the resistance of a conductor of unit length weighing one pound, and b is a term constant for any given installation within certain limits and depending only upon the financial data.

If we take 100 yards as our unit of length, and assume a working temperature of 25° C. and a conductivity of 95 per cent., we shall find that

$$\rho = 3\omega.$$

(Strictly, $\rho = 3{\cdot}08\omega$; but as neither temperature nor conductivity are known absolutely, we may as well take the whole number.)

It may perhaps be thought that in (41) we should rather use the value $C_\theta \sqrt{\frac{T}{t}}$ as representing more nearly the actual

current during the hours of working. We may certainly do this without error, for it will lead to the same value for W. This will be seen on examining the term b.

We have

$$b = \frac{\cdot 00134\, z}{\beta\,(p + a)} \quad \ldots \quad \ldots \quad (42)$$

Here z represents the cost of 1 E.H.P. per annum *developed continuously;* but if we increase C_θ to $C_\theta \sqrt{\frac{T}{t}}$ we shall have to take z for an average of t hours per day, consequently $\sqrt{b}$ becomes $\sqrt{b\frac{t}{T}}$, and in (41) W remains unaltered.

In estimating z we shall of course remember that the engines run q hours only per annum, and therefore we must find the cost of Q hours running at the particular rate per hour found for q hours.

In the above expression for b, β is that fraction of the first cost of the cable which is to be charged annually for interest and maintenance, p is the cost of copper per pound, and a represents an expression for the cost of insulation and protection *per pound of copper in the cable*, so far as this is not a constant sum. a may be of a form which does not admit of the direct determination of W in terms of C_θ, in which case we shall have to prepare tables of the values of C_θ for different values of W, and the value of b will then vary in different parts of the system.

Having found W—the weight of the required conductor per unit length—it is worth while to note that for a cylinder of copper we may put

$$d = 2\sqrt{\frac{W}{l}} \quad \ldots \quad \ldots \quad (43)$$

where d is the diameter in inches, l the length in inches, and W the weight in pounds. In this case, the length being 100 yards, this reduces to

$$d = \frac{1}{30}\sqrt{W}.$$

In (43) the true log. of the coefficient is 0.300935. The formula is useful for finding the *effective* diameter of stranded cables.

LIMITING DISTANCE OF LAMPS.

Professor Forbes has shown that in any installation the distance of any given lamp from the nearest point of constant potential must not exceed a certain limit. Put this equal to L yards, and let e be the maximum variation of potential permissible, let r be the resistance per unit length of the cable, and let this unit length be l yards, then we must have

$$e = C\,r\,\frac{L}{l}$$

$$\therefore L = \frac{l\,e}{C\,r} \quad \ldots \quad \ldots \quad \ldots \quad (44)$$

Now in this expression for L we shall have $l\,e$ constant throughout the system, so that if L is to be constant we shall only require the condition $C \propto r^{-1}$. But this condition is fulfilled only when C is either equal to the thermal current or bears some constant ratio to it, and only then (as will presently appear) while the value of b is constant in the different parts of the system. Professor Forbes, in his recent Cantor Lectures, seemed to imply that it would be sufficient to adjust the extreme variation of potential to the *average* current. But it seems doubtful whether this would be permissible in practice, because when there are any large installations used at intervals the maximum current is likely to be so much larger than the average. It appears to me, therefore, that in (44) C should be taken nearly, if not quite, for the maximum number of lamps which will ever probably be in use at one time. I should propose to obtain this maximum number by taking the total number hung, and deducting from this (1) say, one-half of the "Irregular" lamps, (2) all lamps in Group IA., (3) all lamps in Group I. which are extinguished before any of those in Group II. are lit. In this case the ratio C_θ : C will not be constant throughout the system. Hence the values of L will also differ in different districts.

Let C_1 be the value of this maximum current in the first district, and indicating the particular values of the other terms in (44) by the same subscript, we have

$$r_1 = \frac{\rho}{W_1};$$

so that

$$L_1 = \frac{l\,e\,W_1}{C_1\,\rho}.$$ Substituting for W_1 from (41),

we have $$L_1 = l\,e\,\frac{C_\theta}{C_1}\sqrt{\frac{b}{\rho}} \quad \ldots \quad \ldots \quad \ldots (45)$$

Now in this expression, whatever be the values of the units chosen, we shall have $(l\,\rho^{-\frac{1}{2}})$ constant, and (to give L_1 in yards) equal to 57. So that (45) becomes

$$L_1 = 57 \times (e\,b^{\frac{1}{2}}\,\frac{C_\theta}{C_1}) \quad \ldots \quad \ldots (46)$$

I may, perhaps, be permitted to mention, in conclusion, that I have elsewhere shown that the limits of L_1 are largely extended by the use of "multiple mains" when the relative dimensions of the external and intermediate mains are properly proportioned.

Professor ADAMS: Mr. Snell has dealt with a subject of very great importance in its bearing on the lighting of the future, and in his paper he has entered into considerable detail on the subject. I see there are several present who on previous occasions have given us ideas on the subject, and other practical men whose views we shall be glad to have, so that we may do something towards threshing out this important subject this evening. Perhaps Professor Forbes, who has bestowed considerable attention to this subject, will lead the way.

Professor FORBES: It is quite true, Sir, as most members are aware, that I have devoted a very great deal of time and attention to this special subject; and in fact the main point which Mr. Snell has brought out is one which I believe I was the first to draw attention to in the course of the Cantor Lectures during the spring of this year—that is to say, the modification of Sir William Thomson's law which must be made owing to the irregular manner in which lamps are lighted. I am glad to say that Mr. Snell thoroughly accepts the view that Sir William Thomson's law, properly applied, must regulate the size of conductors in any such system of distribution as that we have been speaking of—that is to say, because Sir William Thomson's law is the law of economy: it simply means putting down the most *economical* conductors possible, and I am very glad to see that

the axiom, as we call it, is being universally accepted in England, that we are to put down the most economical conductor. It seems a truism; and yet, when one has gone through America and seen the way in which this important law is utterly disregarded, and the lavish expenditure of energy in their conductors, in order to save the comparatively trifling expense of laying down suitable conductors for the lamps, one cannot be too glad that in this country wiser counsels seem likely to prevail. In showing the method in which Sir William Thomson's law was to be applied, in the course of those Cantor Lectures I drew attention to the fact that we must not take the total number of lamps on the circuit, and the total current which would be used for meeting the whole of the lamps, as the current for which we have to provide; but that we must take a fraction of the total number of lamps, which depends on the proportion of lamps which are being used at different periods. And it is not a simple average which we can take, because it depends on the square of the current, so that if we simply take the average of the current it would not be at all correct. I went so far in those lectures (though dealing with this subject in a very elementary manner) as to give some of the primary mathematical considerations that must guide us in making the size of conductors depend on irregularity of consumption, and I gave some special cases, with the corresponding amount of the thermal current, as Mr. Snell has called it. I think that Mr. Snell has done us a very great service in showing us in special cases how we can very conveniently apply a rigorous treatment to estimating the size of mains. There may be some here who think that it is a little too finikin, that too much attention is paid to the actual calculations—the numerical calculations—and that we do not require any such accuracy. Now, I cannot impress too strongly upon anybody the importance of economy in the matter of mains. If we are dealing with the multiple arc system, the cost of the mains, as every one here knows, is something gigantic when we come to a large central station lighting; and every step which we can take to introduce economy is introducing a proportionate economy, not only into the copper we *are laying* down, but a proportionate and sensible

economy in the whole cost of the installation. Because the cost of mains is one of such vital importance in the multiple arc system on a large scale, therefore, as I tried to impress on my hearers in the Cantor Lectures, and as Mr. Snell's paper suggests, I repeat that it is most essential that we should, before ever giving out the plans of any installation, calculate out over and over again the greatest economy that we possibly can get from different systems. I assure you that the amount of labour that is required for calculating out the mains for a central station electric lighting is something enormous. You have to calculate it out on various different suppositions, and find out how it varies under different possible conditions that may arise. You have to modify your plans over and over again; and it makes me quite sad to see the reckless way in which estimates have been made at times for central stations, when the mains have actually been calculated, perhaps, in an hour or two—a matter which it is impossible that any one man can do unaided, under two or three months at the very least. It is an enormous labour to calculate out the mains for a central station. But there is one great thing to remember—that it is all calculable, that we do not need to go by guesswork, or rule of thumb, or hap-hazard at all: the whole thing is a perfectly straightforward calculation; and if we will spend the time upon it, and the money, it will be repaid—that is to say, that by devoting the time necessary to get out the most economical system, we are undoubtedly saving the undertakers an enormous amount of money. It is perfectly true that in practically laying down an installation we may not be able to follow out the exact rules which we have arrived at in the theoretical calculations. According to the theoretical calculations we shall be gradually diminishing our mains, tapering them and reducing them in thickness at each point where there is a supply to customers. Now, in practice you can easily see that this is not rigorously possible—that it will be necessary to carry the same size of mains past a good many houses which are drawing a supply from these mains. That is very true, and I can quite *imagine* some people who dislike accuracy saying, "Well, if we *cannot* get that accuracy,—if practice prevents us from estimating

that average,—you need not talk to us so much about it." That is all nonsense. If we are able to lay down no rule of a system which is undoubtedly the most economical, it is a very simple thing to pass on from that to the nearest practical arrangement which approaches to that theoretically perfect arrangement.

Mr. Snell has employed a very ingenious method of getting some approximation to what he calls the thermal current. I should be inclined to think that this method is apt to give us rather wide limits, even following out the system as he has done it; but while we are in the present state of calculation in the matter, it will be of great assistance to all of us who occupy ourselves at all in this matter to have not only this proposition laid down, but as many more propositions which are shorthand ways of arriving at results. Such systems as those proposed by Mr. Snell will very probably be largely used,—at any rate in certain cases,—and even if it be not the most accurate way, at any rate it is admirable as a check; and in calculations which involve such an enormous amount of labour as I have spoken of, it is very desirable to have shorthand methods of arriving at checks, so that every part of the calculation need not be gone into by each person who checks it, and such that slips are not likely to occur in that way. I must say that I should have thought—my own impression has been—that the best way, after we have arrived at some notion of the consumption, is simply to attack the problem directly when we vary the consumption at different hours, and find out the waste of energy at each different hour during the twenty-four; and if it varies during the days of the week, find out also for the different days. Still, I must say, this method of arriving at a maximum and minimum is extremely beautiful, and one which I think will be used by those of us who are employed in this work.

Just now I spoke of the enormous amount of labour and calculation that we have in estimating the size of mains. We have various stages to consider and to go into, not only because there are different assumptions as to the number of lamps which will be used, but also because there are different means of laying our mains; even if we confine ourselves to a multiple arc system, there is a large *number* of ways of laying them. Among these, I

pointed out in the course of the Cantor Lectures in the spring, there is one which employed distributing boxes such as Mr. Edison has used in his central station in New York, but by no means in the manner in which he has put down these distributing boxes. The employment of these boxes would be a great economy in most cases; but, at any rate, even in multiple arc systems we have different methods of laying down the mains, and we must calculate out the economy of laying them. And not only do we have these different methods, but systems have been introduced lately, some only on paper, for using higher potentials, for using multiple arc series, and so forth, and eventually we may have secondary batteries perfected in order to use the higher potentials. All these, too, ought to be calculated out; and in taking account of the disadvantages of a high-potential current or of a multiple series, against the multiple arc, we must also take into account the enormous advantages, and therefore the economy of such a system has to be calculated out, and the whole advantages in one manner and another must then be weighed in the balance. I think I have said enough to show what a great deal of labour must be taken in order to make electric lighting a financial success, and it is my own opinion that Mr. Snell has largely helped us in simplifying the labour which will be required to calculate out the mains in any central system of lighting.

Dr. Fleming: It is impossible, Sir, to do justice, in the way of criticism, to this exceedingly interesting and valuable paper to-night, seeing that we only had it placed in our hands as we came into the room, and during its reading we have been able to do little more than follow the general argument which Mr. Snell has put before us.

The quantity to which Mr. Snell has given the appropriate name of the "mean annual thermal current" is a very important quantity indeed, and Professor Forbes very justly laid great stress upon it in his Cantor Lectures. In fact this quantity, in its relation to distribution, is very much like the mean square of velocity in considering a problem in molecular physics. The *square* of this mean annual current multiplied into the *effective* resistance of the network will therefore represent at

once the energy dissipated per second. In Lord Rayleigh s work on "Sound" he has given a useful name to the quantity which expresses the rate of dissipation of energy per second in any system: he has called it the "dissipation function;" and in any network of conductors having a current running through them the dissipation function of that system is the square of this mean current multiplied into the effective resistance of that system. That is the rate at which the heat is being got rid of per second. Now, when I was in New York, I tried to find out, if possible, what the "mean annual lighting current" really was; and I was able to do this by favour of getting the meter returns for six months, and also by getting the ammeter readings taken on the machines at every half-hour during the whole time, and in this way found what was the amount of current in ampère hours running through all the lamps in six months, and to obtain the mean annual lighting current for that time; and it will probably interest Mr. Snell and others to know what a comparatively small fraction that current was of the whole current that the installation was capable of putting out. In the station there were 12,600 lights installed (this number has been considerably increased since then), and statistics show that the maximum number of burning lamps at any time was 5,000, and the minimum number at any time only 100, coming somewhere about two or three o'clock in the morning. Now, taking the meter reading for that time, and obtaining the total quantity of electricity going out of the station, it was possible to calculate what this mean annual current was, and it was only equal to 1,200 lamps, or one-tenth of the capacity of the station. It will therefore be interesting to some to know what this mean current is in the case of an actual installation. But the one thing which does present great difficulties in all practical cases is not the actual calculation—because that is possible—but in ascertaining the distribution. That is where the real difficulty lies. You have an idea of what you propose to put down, and can ascertain the number of lights, but you do not know how many lights A, B, C, and D will take; you have to make a guess. It is only by *experience that you* will be able to make reliable estimates

for distribution of lamps in central stations. We shall have to go very slowly in this matter. Experience will guide us in making the right kind of estimates.

Nevertheless I agree with what has fallen from Professor Forbes—that the matter has received very great assistance from those valuable and exceedingly interesting investigations which Mr. Snell has brought before us.

Mr. SHOOLBRED: I am afraid, Sir, I can hardly contribute anything to the discussion of this valuable paper, but I will ask, if you will allow me, a question with regard to what is now generally accepted, as Mr. Snell has stated, as to the value of Sir William Thomson's law of economy in conductors. There is, I think, and especially if I may judge from Professor Forbes' remarks, some doubt as to the exact interpretation to be put on the law that Sir William Thomson has stated. In its original state, as many persons interpret it, Sir William Thomson is supposed to have placed the limit of fifty ampères per square centimètre as the most economic value of the conductors. A good deal of exception has been taken to that as requiring an enormous proportion of conductor. Professor Forbes, I think, advocated in the course of Cantor Lectures recently delivered before the Society of Arts, and already referred to this evening, a section of conductor of about one and a half square inches per thousand ampères, which would bring it practically to about double Sir William Thomson's limit. The limit indicated by Professor George Forbes coincides very closely with one advocated, I understand, by Dr. J. Hopkinson, viz., one ampère per square millimètre.

Professor FORBES: I was not specially advocating that size. I simply showed that under certain assumptions Sir William Thomson's law would give that size—from certain assumptions that seemed natural.

Mr. SHOOLBRED: Exactly; but I know of no large installation where Sir William Thomson's interpretation of 50 ampères per square inch is carried out. I know many to the contrary, where the installations have been laid down very successfully upon the *other limit* just referred to. To those who are engaged in the *laying down* of electric light installations a true economy in the

conductors is of great importance, as has been repeatedly urged by Professor George Forbes. In my own practice I have made use of a working limit of about 700 ampères per square inch, which coincides closely with Dr. Hopkinson's limit; and I have never found occasion to regret it. It would, however, be satisfactory to clear up the doubt as to the exact interpretation of Sir William Thomson's law.

With regard to Mr. Snell's remarks about the extreme difficulty of ascertaining accurately as to the number of lights which will be required in actual practice, I think we must agree on that point with Dr. Fleming—that in the first instance it must be a matter of guesswork at most. Of course we should, nevertheless, endeavour to arrive at these requirements to the best of our power; and then, in designing the capacity of the conductors, we should take advantage of the theoretical methods laid before us by Mr. Snell, Professor Forbes, and others.

Professor ADAMS: I am sorry that we have not had the experience of some of our members who have been engaged in this subject. I am afraid there has been somewhat of a check in the laying of mains and actual distribution, and that practical men are not taking that interest in the development of the theory of the subject which they would take if the matter were pressing, and if next week a very important installation were going to be made. If no other member wishes to take part in the discussion, I will call upon Mr. Snell to make any remarks he may wish to add to his paper.

Mr. SNELL: I am glad that I have not to reply to any hostile criticism this evening, and I am very grateful for the kind way in which the paper has been received.

Dr. Fleming has indicated what is no doubt the great difficulty in making use of such calculations—that is to say, that we are not called upon to supply electric lighting to the whole of a given district, but only to such inhabitants as may choose from time to time to take it. It seems to me, however, that we might fairly lay out our mains upon the basis of being prepared *ultimately* to do very nearly the same amount of work as is now done by the gas *companies—that is,* so long as we are not to be deprived of

the property at the end of twenty-one years. By that time it may be hoped that a great deal of gas will have been finally extinguished.

Professor ADAMS proposed a hearty vote of thanks to Mr. Snell for his exceedingly valuable and interesting paper, which was cordially given.

He then announced that the President proposed to invite members to visit the Swindon works of the Great Western Railway Company on 30th June.

The meeting was then adjourned until November 12th.

DEAR SIR,

Kindly note that in my paper on "Earth Currents in India," dated 16th September, 1884, there is a printer's error in the 15th line of "divisional" for "diurnal."

Mr. Chambers, from whose observation a quotations appears in the paper in question, writes that the quotation refers to plates not given in my paper which "represent the lunisolar variation of horizontal force. The range of this (·00054) is, however, less than a tenth of that of the solar diurnal variation (·00611), and it is very different in character."

Yours faithfully,

E. O. WALKER.

ABSTRACTS.

(We are indebted to the courtesy of the Institution of Civil Engineers for allowing the first five of the following Abstracts to be reprinted from their Proceedings.)

G. CHAPERON—ON ELECTRO-DYNAMOMETERS.

(L'Electricien, Vol. 8, *No.* 85, *pp.* 337-442, 1884, *and No.* 86, *pp.* 386-389.)

The author points out that the instrument most frequently employed for the industrial measurement of alternate currents is the electro-dynamometer. The mechanical action between its fixed and movable coils is sufficiently strong, and the essential condition for procuring comparable results in various cases is attained, viz., that this action can be easily measured or balanced without employing coils with a large number of convolutions. If used, however, as a voltmeter for measuring the difference of potential of two points in a circuit, the instrument must have a high resistance, which can be obtained either by diminishing the section of the wire composing the coils, or increasing the number of convolutions. In the former case, the current traversing the dynamometer becomes very feeble, and the mechanical action, which varies as the square of the current, soon ceases to be perceptible, and in the latter case, the coefficient of self-induction is considerably augmented, and the instrument can only be used for comparing variations of potential of the same period and form. The author refers to the investigations and formulas of Mr. Joubert, which show that, in the construction of an electro-dynamometer of high resistance, the difficulty is to augment this resistance without increasing in the same ratio the coefficient of self-induction, and diminishing the sensibility too much. In order to overcome this, the author suggests that the electro-dynamometer might be composed of two flat discs spirally covered with fine wire and mounted very close together, as in the electro-dynamical balance of Mr. Lallement. If a current is made to traverse the two spirals in contrary directions, the coefficient of self-induction for the whole system will be very feeble, but the repulsive force of the discs will be so strong that it can be measured by the torsion of a spring or other method. Instead of measuring this force, the deflection of a light coil, or a piece of soft iron of small magnetic inertia suspended between the discs, can be read off. The second plan would be preferable, if the discs were required of such weight and volume that the suspension of one of them would prove inconvenient. In place of the discs, two flat bobbins in close contiguity can be employed, and so arranged that a *movable system* can be easily suspended between them.

In an apparatus of this kind, it is important to know what error has been introduced by neglecting the ratio, or rather the square of the ratio, of the coefficient of self-induction to the resistance. The author describes the method adopted by him for determining the limit of this error, and the coefficient by which any current passing through a given coil must be multiplied to obtain the total quantity of electricity traversing the wire during the period of the extra-current or time during which this current must circulate in the coil to furnish the same amount of electricity as that due to the final impulse.

In a continuation of the paper, the author gives diagrams descriptive of the type of electro-dynamometer devised by him. In the directing field, formed by two fixed coils placed close to each other, and arranged so that the current traverses them in opposite directions, is suspended bifilarly a disc of varnished cork wound spirally with about fifty convolutions of wire. The position of equilibrium of the disc must be made to coïncide with the plane of separation of the coils. By means of a mirror fixed to the disc, the deflection of the latter can be determined and the strength of the directive force evaluated. For leading the current into the movable circuit, the two suspension wires may be employed, as in Weber's apparatus, but as their length must be considerable, in order to avoid heating, this method is inconvenient. Mercury contacts in the case of instruments of high resistance are difficult of adjustment. The author has found that the following device gives good results: the two ends of the wire forming the movable coil are soldered to two fine copper wires, shaped like a two-pronged fork; the extremity of each prong dips into a jar filled with a saturated solution of cupric sulphate, the current being led from the fixed coils to copper electrodes suspended in the jars. As the instrument is employed for the measurement of alternate currents, no inconvenience arises from the electrolyte.

This form of movable circuit is absolutely astatic, and therefore insensible to terrestrial magnetism, or even to any more powerful magnetic field in which it may be placed. The author has also employed for the movable system a light disc of soft iron, making an angle of some degrees with the plane of separation of the two fixed coils. With a disc 0·1 millimètre thick pierced in the centre, and having radial slits, sensible deflections are obtained from a current of less than the two-thousandth part of an ampère, or an electro-motive force of half a volt at the terminals, the coils being joined together in derived circuit.

F. EVRARD—ELECTRIC FIRE-ALARMS.

(*La Lumière Electrique, Vol.* 13, *p.* 85, 1884.)

The electric fire-alarm systems of different countries are classed by M. Bartelous under the following heads:—

1*st System.*—Giving the alarm to a fire-station or to a police office, which is in telegraphic communication with the branch and head stations of the fire-brigade.

2nd. Electric communication from the various police stations to specially

constructed alarm boxes, which may be worked automatically or require to be put in action by an official of the force.

3rd. Private apparatus, enabling one to give the alarm from one's own house.

4th. Automatic apparatus, giving the alarm on pyrometrical principles.

5th. Application of electricity to accelerate assistance.

Nearly all large towns come under the first category, and have their police offices communicating directly with the chief and branch fire-stations. To make this system thoroughly efficient there should always be an alarm-office within 300 yards. The telegraphic system has the advantage of preserving a record of the indication and orders given, by which means it is easy to ascertain who is responsible for the alarm.

In Paris the chief station of the fire-brigade is connected, not only with the branch stations and police offices, but also with each of the following:—

1. The head office of the water-supply.
2. The head-quarters of the "*Assistance Publique.*"
3. The "*Préfecture de Police.*"
4. All telegraph offices.

It has also been proposed by the "*Union Syndicale*" that messages relating to fires should be transmitted free of charge by all telegraph offices.

The system which comes under the second heading is that employed to a great extent in Germany (Frankfort-on-the-Main, Munich, Hamburg) and also at Amsterdam. In Belgium, Verviers alone has a system of the kind, though Brussels has lately adopted it in theatres and public buildings. New York possesses a great number of alarm boxes. These can, however, only be opened by numbered keys furnished to responsible persons, and when the alarm has been given the key cannot again be withdrawn without the use of a duplicate, the possession of which is confined to the police and fire-brigade, thus preventing all false alarms.

Members of the Stock Exchange and other people in London who are subscribers to private telegraph and telephone companies, have the power of obtaining assistance through these channels, under the third system.

Under the fourth heading come a number of apparatus working on the principle of expansion. When a fire breaks out the heat generated causes the completion of a circuit by expanding some part of the apparatus so as to come in contact with another portion, and in this way the alarm is given. There are, however, many disadvantages in this system. To make it properly efficient a great number would be required, whilst their liability to get out of order would necessitate a great deal of looking after. They would be of great use, however, if placed near enough to the possible sources of a fire, or where there was a danger of spontaneous combustion.

A very ingenious system in use in the United States may be mentioned under the fifth heading. On the alarm being given the current causes the doors of the fire-station to open, the horses leave the stable and place themselves before the engine, the harness, which is suspended over it, drops on to them, and in five seconds the engine dashes out Thus in less than a minute

after the alarm has been given the first engine might be on the spot. It was next thought that, by the use of water on the point of ebullition, some of the time required to get the engine under pressure might be saved. The five minutes following the alarm are, in the opinion of the heads of the Metropolitan Fire Brigade, more important and valuable than the five succeeding hours. To save a few minutes at this critical moment the San Francisco brigade have a boiler in the basement which is connected, by means of an inlet and outlet pipe, with the boiler of the fire-engine, and kept in a constant state of ebullition. The connection between these boilers is broken off by the current which gives the alarm.

J. MUNIER—TELEGRAPHIC APPARATUS.

(*La Lumière Electrique, Vol.* 13, *pp.* 379-382, 1884.)

This is a corrected statement of the work obtained from telegraphic apparatus actually in use in the French service. The author makes two great divisions, into signalling and printing apparatus, and gives the following list of systems in order of speed:—

	Number of characters a minute.	Theoretical production of messages.	Practical production.
1. Baudot (sextuple)	780	360	200 to 250
„ (quadruple)	520	240	130 „ 180
2. Meyer (octuple regulated to 90 revolutions)... ...	720	332	200 „ 250
Meyer (sextuple)	600	276	180 „ 200
„ (quadruple)	400	184	120 „ 150
3. Wheatstone	300	138	120
4. Hughes	175	80	60 to 70
5. Morse	75	35	25 „ 30
6. Needle	50	23	18 „ 20

The number of forty to fifty messages attributed to the Hughes system is quite erroneous, as is that of seventy-five to Meyer's. A Meyer octuple apparatus produces as many, if not more, than a Baudot sextuple, similarly as a Meyer sextuple will produce more than a Baudot quadruple. The sole advantage of the Baudot is that it prints.

If the number of emissions of current to produce a given number of characters be the standard, the Hughes is far in advance.

To produce one character in each system requires—

In Hughes system	1	emission	of current.
„ Meyer „	3 to 4	„	„
„ Morse „	4	„	„
„ Baudot „	5	„	„
„ Wheatstone	6	„	„
„ Needle „	6 to 7	„	„

For signalling apparatus, in order to obtain the average of emissions, it is

necessary to know the average presentation of each letter in the formation of words, and this is for—E 219, R 118, N 108, A 107, S 106, I 105, T 98, U 82, O 80, L 69, D 52, C 48, P 46, M 46, É 39, V 27, G 17, H 17, F 15, Q 15, B 14, X 8, Y 6, Z 6, J 5, K 1.*

Given a wire capable of three hundred thousand emissions an hour, and a mean of one hundred and thirty emissions a message—

The Hughes should give	2,308 messages.
„ Meyer „ „	666 „
„ Morse „ „	576 „
„ Baudot „ „	462 „
„ Wheatstone „	385 „
„ Needle „ „	355 „

Despite the most ingenious mechanical arrangements, the producing power of all the apparatus remains inferior to that of the lines.

The author terminates his remarks by a statement of the production of each apparatus actually in use per hour and per employé.

	Number of messages an hour.	Total number of employés occupied at the two ends of the wire.	Mean number of messengers.
Baudot sextuple	240	16 (12 operators, 2 writers, 2 directors)	$\frac{240}{16} = 15$
Meyer sextuple ...	180	12 operators (including 2 directors or superintendents)	$\frac{180}{12} = 15$
Wheatstone	120	8 (6 operators, 2 directors)	$\frac{120}{8} = 15$
Hughes	60	4 operators	$\frac{60}{4} = 15$
Morse...	30	2 „	$\frac{30}{2} = 15$
Needle	20	2 „	$\frac{20}{2} = 10$

It seems that, whichever the system used, the number of messages per employé is practically the same. With the high speed apparatus of Baudot, Meyer, and Wheatstone, the average is reduced by the presence of special employés, termed directors, who keep the apparatus in working order. These directors are more or less necessary; the Baudot apparatus especially refuses to work if he be absent half an hour. This is a great inconvenience, and points to considerable improvement still to be effected.

Dr. A. TOBLER—NOTE ON A CONDENSER.

(La Lumière Electrique, Vol. 14, pp. 486-488, 1885.)

The condenser in question was constructed by Messrs. Berthoud, Borel, & Co., of Cortaillod, Switzerland, and was of the nominal value of 1 microfarad.

* *This is, of course, for the French language.*

According to the Report on the Electric Exhibition of Munich,* its dielectric consists of paper, treated with a mixture of resin (colophonium) and oxidised linseed oil. This material presents the special advantage of retaining the smallest possible residual charge, that is, its absorptive power is exceeding low.

The author carried out investigations as to the following—influence of duration of charge; of charging potential; insulation; residual charge; value of subdivisions.

Influence of duration of charge:—

P = 2 Daniell elements.

Capacity = 1 mfd. T = 13·5° C.

Duration of charge.	Deflection.
2 seconds	166
5 "	166·5
10 "	167
20 "	167
30 "	167
120 "	167

At the end of ten seconds, therefore, the condenser took its maximum charge.

As to the influence of the charging potential, the author at first experienced some differences, although identicality of cells was carefully verified; but by charging from the same battery shunted with three resistances of 5,000 ohms each, at different points of the circuit, nearly perfect concordance was established with proportionality to potential.

Charged with 2 Daniells, the maximum loss of charge in one minute was 1·5 per cent., a result said by the author to be rarely attained.

Time of charge.	Immediate deflection.	After 1 minute.	Percentage loss.
10 seconds...	167	164·5	1·5
15 "	"	165	1·2
20 "	"	165	1·2
30 "	"	166	0·6
40 "	"	166	0·6
50 "	"	166	0·6

It is curious to note that the loss diminishes with increasing times of charging.

For residual charge, the condenser was charged during ten seconds, discharged during a given time, insulated for two minutes, and again put into connection with the galvanometer.

P = 2 Daniells; Cap. = 1 mfd.; Temp. = 16° C.

Deflection.	Time of discharge.	Charge remaining after 2 minutes' insulation.
1,700	1 sec.	10
"	10 "	4
"	15 "	2
"	30 "	1
"	60 "	0·75

* "Officieller Bericht eiber die Eleketricitats-Austellung in München," 1883, pp. 82 and 151.

H. HESEHUS—ON AN AMPÈRE-METER BASED ON THE PELTIER EFFECT.

(*Exner's Repertorium der Physik, Vol.* 21, *p.* 151, 1885.)

This instrument consists essentially of a differential air-thermometer, the two bulbs of which enclose the opposite ends of a thermopile; so that when an electrical current is passed through the thermopile it traverses the junctions in each bulb in opposite directions; and as a result of the Peltier effect there will be a cooling at one set of junctions, and a heating at the other; the action of this on the gas enclosed in the bulb will cause a movement of the mercury index in the fine tube which connects the two bulbs. The heating of the bars themselves by the current will be equal in each tube, and therefore such effect will be balanced.

For alternate currents, the instrument can be used as an ordinary air-thermometer, and the movement of the index will then be proportional to the square of the current.

In an instrument of this type constructed for the author, twelve bars of iron and nickel, soldered together to form two pairs of series, are bound together with cement, so as to form a solid prism, and on to its ends were cemented glass vessels, each entered by two small tubes, one in each communicating with the U tube containing the mercury index, and the others through a pinch cock with the outer air. By comparison with one of Ayrton and Perry's ammeters, a movement of one division of the scale indicated a current of 0·68 ampère.

The following advantages are claimed for this form of instrument: that it gives the intensity of the current at each moment, thus superior to a voltameter; it can be simply and cheaply constructed; it can be used, as indicated above, for either direct or alternate currents; and, especially, that it is absolutely unaffected by any magnetic field, and can therefore be used in close proximity to dynamo machines.

CAILLETET and BOUTY—CONDUCTIVITY OF SOLID MERCURY AND OF PURE METALS AT LOW TEMPERATURES.

(*Comptes Rendus, Vol.* 100, *No.* 19, *May* 11, 1885, *p.* 1188.)

The experiments of Matthiessen and Benoit have shown that for temperatures between zero and boiling point the mean coefficient of increase of resistance per degree does not differ materially for different metals, and it is represented very closely by the fraction $\frac{1}{273}$, the coefficient of expansion of gases. It was therefore interesting to see if the law held good below zero, since, if the resistance continued to decrease by the same quantity, the resistance of a metal, varying as the pressure of a perfect gas at constant volume, would serve as a measure of the temperature, and would become nothing at the absolute zero of temperature.

The authors have carried out experiments with mercury enclosed in spiral glass tubes surrounded by freezing mixtures or by methyl chloride; also with *other metals wound into hollow* coils, which could be immersed in a similar

way. The temperatures were read on a hydrogen thermometer. The measurements of resistance were merely comparative, and are not given in ohms, the standard of comparison having been a column of mercury at zero.

The following are the results obtained:—

1. *Mercury.*—The formula for mercury above zero can be applied down to the point of solidification (− 39·5° C.); the metal then becomes a much better conductor, and at − 40° C. it is 4·08 times better than the liquid metal. The resistance of the solid mercury decreases regularly, and between − 40° C. and − 92° C. it is given by the formula

$$R_t = R_{-40} \left\{ \frac{1 + a\,t}{1 - 40\,a} \right\},$$

where the coefficient $a = 0{\cdot}00407$, which is about five times that for liquid mercury, and nearly approaches that for other metals.

2. *Silver, Aluminium, Magnesium, Tin.*—The same formula holds good for these four metals, viz.,

$$R_t = R_0 \, (1 + a\,t),$$

where a has the following values:—

Silver	0·00385
Aluminium	0·00388
Magnesium	0·00390
Tin	0·00424

3. *Copper*—A much more complete series of experiments than with the other metals led to the values of a.

Limits.	
0° to −58·22°	0·00418
−68·65° to −101·30°	0·00426
−113·08° to −122·82...	0·00424

These values are higher than those previously found, viz., Matthiessen 0·00367, and Benoit 0·003637.

4. *Iron, Platinum.*—These two metals are not so regular in their behaviour. For iron the formula given above (sec. 2) holds good from 0° to −92° with $a = 0{\cdot}0049$. For platinum at 0°, a = about 0·0030, which becomes 0·00342 at −94·57°, showing that platinum approximates to the other metals at very low temperatures.

DELPIEU—FIRE-DAMP ALARM.

(*Comptes Rendus, Vol.* 100, *No.* 20, *May* 18, 1885, *p.* 1417.)

The apparatus described depends for its action on the changes of density of a mixture of air and marsh gas, the proportions of which vary. It is an improvement on an original apparatus submitted to the Academy in August, 1881.

BENOIT—CONSTRUCTION OF COPIES OF THE LEGAL OHM.

(*Journal de Physique, Vol.* 4, *Jan.*, 1885, *p.* 5.)

After the Conference of 1884, at which the legal ohm was settled, the author was requested by the Minister of Posts and Telegraphs to make copies of the

unit of resistance. This he was able to do with great accuracy, having placed at his disposal all the resources of the International Committee of Weights and Measures.

To calculate the resistance of a column of mercury, it is necessary to know (1st) the internal shape of the glass tube, (2nd) its capacity, (3rd) its length; we can then use the formula,

$$R = r\frac{l^2}{v}\left\{\left(\frac{1}{C+x_b-x_a}+\frac{1}{C+x_c-x_b}+\ldots\ldots\right)C^2+\frac{(n-m)^2}{n-m+x_n-x_m}\right\},$$

where r is the resistance of a column of unit length and unit section; l is length at zero of one division of the graduated tube; v the capacity of one division; a, b, c, &c., the several divisions; C the constant value $b-a=c-b=$ &c.; x_a, x_b the corrections to be made for the individual divisions.

In order to introduce the mercurial column into the circuit, the connections were made by means of large flasks full of mercury, into which the tube entered through lateral tubulures. This method of connection introduces an additional resistance which must be taken into account. It is obtained by adding to the length of the tube a quantity equal to 0·82 times its diameter. The coefficient 0·82, which had been deduced by Lord Rayleigh from theoretical considerations, has been experimentally verified by Mascart and De Nerville.

The author made use of four tubes, two of green glass, two of flint glass. They were all about 1·2 m. long, with a sectional area of 1 square mm., and were divided into millimètres for a length of 1·05 m. They had been prepared fourteen months before they were actually used.

The calibration of the bore of the tubes was most carefully carried out in the same manner as is done in the case of delicate thermometers, viz., by accurately measuring the length of a thread of mercury at various points of the tube.

To determine the capacity of one division of the tube, it was filled with a column of mercury 950 to 1,000 mm. long, which was in like manner measured in twenty different positions, allowance being made for errors in the size of bore as determined by the preceding operation, and also for the meniscus. The length having been thus most carefully ascertained, the mercury was emptied into a small glass vessel, and weighed with all the nicety possible with the very delicate instruments of the Committee of Weights and Measures. Adopting the value 13·5956 for the specific gravity of mercury, it was then easy to calculate the volume comprised between any two divisions of the tube. It was found that in the measurements of lengths it was sufficient to divide the tube into twenty-one sections, as this gave the true resistance within $\frac{1}{100000}$. Starting from the zero division the exact length of each tube was determined which should have a resistance of one legal ohm, and by successive trials the tube was reduced to this length. The whole of the measurements having been made at the prevailing temperature of the room, the coefficient of expansion of the glass was obtained, and the corrections made to bring the values to zero.

The geometrical determinations having been completed, the resistances of the four tubes *were compared* electrically amongst themselves. The bridge

was made up of three German silver wire resistances, each nearly one ohm in value, the fourth branch containing the tube under test. The apparatus was completed by a stretched wire with a sliding contact. A displacement of 1 mm. on the bridge wire caused a deflection of about 300 mm. on the scale of the galvanometer. The value of the bridge wire had been accurately determined beforehand, and it was found that 1 mm. was equivalent to 0·0000907 ohm. The comparisons were made between each pair of tubes by two observers, the author and Mr. De Nerville.

Admitting that the mean value 0·999994 ohm as deduced from the geometrical measurements is exact, the resistances of the four tubes come out—

	Benoit.	De Nerville.
I.	1·000016	1·000020
II.	0·999996	0·999996
III.	0·999962	0·999959
IV.	1·000002	1·000004

More easily portable copies of these four standards have also been made in glass tubes coiled into a spiral; the ends being reduced until the resistance of the spiral was made accurately equal to one legal ohm.

With reference to the point of the purity of the mercury employed, Mr. Benoit remarks that after several tests it was found that mercury purified repeatedly by the action of hot nitric acid, then dried under a layer of concentrated sulphuric acid, and finally passed over caustic potash, was always directly comparable, from whatever source it might have been derived.

H. PELLAT—CAUSE OF THE ELECTRIFICATION OF CLOUDS.

(*Journal de Physique, Vol.* 4, *Jan.*, 1885, *p.* 18. *La Lumière Electrique, Vol.* 16, *No.* 18, *May* 2, 1885, *p.* 236.)

When the potential of an insulating medium increases as we recede from a conductor in its midst, this latter is negatively electrified; and this is the case with the air in fine weather, so that Peltier's *hypothesis* that the ground is normally covered with a layer of negative electricity can be shown to be a *fact*. Even if some portions of the earth's surface are positively electrified, as is indeed the case, they are so limited in extent as to leave the balance of the earth's charge of negative sign.

A cloud arising near the earth's surface may be in electrical communication with it, and so become negatively electrified, and on the dispersion of this cloud by evaporation the surrounding air would take up this negative charge. A cloud is a sufficiently good conductor to allow of the potential at all points in it being practically the same. If, then, a cloud is formed in an orignally clear sky, this cloud will be electrified positively below, and negatively above. If now by the action of the wind the two portions of the cloud become separated, we have two clouds charged with opposite signs. Not only can this electrification by induction be shown to take place, but it may be further shown that the quantity thus produced is sufficient to account for *electrical storms*, for as the clouds rise their potential increases enormously,

probably by one C.G.S. unit for each mètre of elevation, so that the mere displacement of clouds may give rise to differences of potential corresponding to several thousand units.

The author therefore reduces the electrification of clouds to the existence of a negative charge of electricity on the earth's surface, which he considers to have always existed since its formation.

ANON.—INSULATING SUPPORT FOR EXPERIMENTS IN STATICAL ELECTRICITY.

(*La Lumière Electrique, Vol.* 16, *No.* 18, *May* 2, 1885, *p.* 247.)

Before the introduction of glass supports, kept dry by the use of concentrated sulphuric acid, it was customary to use stands which were kept warm in order to prevent the deposition of any moisture. Mr. de Fonvielle has proposed to return to this former method, and he has introduced a form of glass support carried on a glass tube, into which is sealed a platinum wire coil which can be readily heated by a current, and by this means the glass may be kept warm and the deposit of moisture prevented. An illustration of the stand is given in the original article.

BARTOLI and PAPASOGLI—ELECTROLYSIS BY DISCHARGE OF LEYDEN JARS THROUGH CARBON ELECTRODES.

(*Beiblätter, Vol.* 9, *No.* 3, 1885, *p.* 183.)

By repeated discharges, about six millions, the carbon electrodes were not disintegrated, but were only slightly corroded at their points. With powder no reaction took place, although the currents would have sufficed to produce several cubic centimètres of explosive gases.

LENZ—USE OF THE TELEPHONE FOR MEASURING TEMPERATURE.

(*Beiblätter, Vol.* 9, *No.* 3, 1885, *p.* 192.)

One junction, *a*, of a thermo-electric couple is kept at a constant temperature, the other is placed at the point of which the temperature is to be determined. An induction coil and a silent contact-breaker are joined up in the circuit. The junction, *a*, is then heated and cooled until silence is obtained, and the mean of the two observed temperatures of *a* is taken.

MICHAELIS—CONDUCTIVITY OF IMPURE MERCURY, AND METHODS FOR CLEANING IT.

(*Beiblätter, Vol.* 9, *No.* 4, 1885, *p.* 267.)

The increase in conductivity of mercury when other metals are mixed

with it is given, if we adopt Siemens's hypothesis that the conductivity, l, of the amalgam is the mean of the mixed volumes, by the formula

$$l=1+\frac{v}{100}(l'-1),$$

where l' is the conductivity of the added metal. The percentage by weight of the added metal in the amalgams considered amounted, at most, to 2·3 per cent. The values obtained for each amalgam do not strictly follow the increase of percentage of metal in it, but are irregular. The results in general agree with those of Matthiessen with the exception of the bismuth amalgam.

The conductivities of the metals dissolved in mercury vary within narrow limits, and are smaller than those of the pure metals. Bismuth is an exception, since the bismuth amalgam conducts better than mercury, though this latter conducts better than pure bismuth. The amalgams of potassium, sodium, and magnesium are difficult to work with, owing to oxidation. The immersion of amalgamated copper wires into mercury has very little effect on the conductivity of the latter, as the copper dissolves excessively slowly.

For purifying mercury, the author recommends boiling the mercury in concentrated sulphuric acid to which a few drops of concentrated nitric acid have been added, then to place the mercury in very dilute nitric acid, and to shake it up till it separates into drops. For the distillation of small quantities of mercury, the author uses a glass tube bent at an obtuse angle, one limb being surrounded with a vessel to admit of its being heated, the other wrapped in a wet cloth, the whole being in communication with a pump for exhausting the air.

LIPPMANN—ELECTRO-MOTIVE FORCE OF AMALGAMATED ZINC.

(*Beiblätter, Vol.* 9, *No.* 4, 1885, *p.* 269.)

Amalgamated zinc in solution of zinc sulphate is electro-positive to ordinary zinc, and is therefore dissolved. According to Robb, pure electrolytically deposited zinc in a neutral solution of zinc sulphate (obtained by addition of barium carbonate) shows no electro-motive force when opposed to amalgamated zinc. In fact, if a current did exist, the amalgamated zinc, since it is electro-positive, would dissolve, the mercury would be separated from it, and the zinc thrown down on the other zinc plate. It would therefore be possible by means of the small expenditure of work of carrying over the zinc to reconstruct the element as often as desirable, which is contrary to Carnot's principle.

The couple cadmium—amalgamated cadmium in solution of cadmium-sulphate behaves differently, and gives a considerable electro-motive force. In this case the amalgamated cadmium is electro-negative, and the action is reversed.

P. CULMANN—EXPERIMENTAL DETERMINATION OF THE SELF-INDUCTION OF A COIL.

(*Beiblätter, Vol.* 9, *No.* 4, 1885, *p.* 279.)

The current from a constant battery divides between a galvanometer and a shunt circuit. At a fixed instant the shunt circuit is interrupted, and then

the galvanometer circuit. If a coil, of which P is the self-induction, is connected up in the galvanometer circuit, P can be calculated from the throw of the needle. For this the exact interval of time between the interruption of the shunt circuit and of the main circuit must be determined. For this purpose the author uses a pendulum, which admits of a direct determination, or Pouillet's method might be made use of.

MENGARINI—METHOD OF DETERMINING THE VALUE OF THE OHM IN ABSOLUTE MEASURE.

(Beiblätter, Vol. 9, No. 4, 1885, p. 280.)

A magnet is allowed to fall, under the action of gravity, through the centre of a coil of insulated wire, in circuit with which an electro-dynamometer is joined up. By means of an apparatus similar to Atwood's machine, the velocity of the magnet at any point of its fall is measured, both when the circuit of the coil is open and when it is closed.

If M is the sum of the masses of the magnet and of the other bodies put in motion, t_0 and t_1 the times in which it passes through a space S, with the circuit open and closed, g the acceleration, and k a constant representing the resistance of the air, then the work done by the magnet in exciting an induced current in the coil is

$$W = \frac{M}{2} S^2 \left\{ \frac{1}{t_0^2} - \frac{1}{t_1^2} + \frac{g}{k} \left(\frac{1}{t_0} - \frac{1}{t_1} \right) \right\}.$$

The induced current has a definite phase, which is made up of two equal quantities with opposite signs. It is therefore measurable by the deflection of the electro-dynamometer. It will be seen that the measurements to be made are almost all mechanical, and that no value of the earth's magnetism enters into the equation.

J. B. BAILLE—DETERMINATION OF THE OHM BY THE DAMPING METHOD.

(Beiblätter, Vol. 9, No. 5, 1885, p. 355.)

The magnet was placed in a stirrup of rather thick brass wire provided with a mirror, and hung inside a circular coil. A long horizontal rod attached to it served for carrying weights, which could be added to alter the moment of inertia. For greater safety in determining the constant of the apparatus, a thermo-current was passed through the coil, and a very carefully constructed tangent galvanometer. From the results of a first series of experiments the author deduces for the ohm the value 105·57 cm. of mercury at 0° C., with an area of 1 mm². A second series led to 105·67, so that, as in all the newest determinations by the damping method, the value comes out smaller than that obtained by other methods.

KRIZIK—ATTRACTION OF IRON CORES BY SOLENOIDS.

(Centralblatt für Elektrotechnik, Vol. 7, No. 7, 1885, p. 136, and No. 8, 1885, p. 162.)

The apparatus used consisted of a stand provided at the top with a pulley, over which passed a cord, the one end of which was attached to the iron core, the other to a spiral spring, which could be stretched more or less by means of a screw and fixed nut. The stand also carried the coil, which could be clamped at any desired position. When a current from 50 thermo elements, equal to about three large Bunsen cells, was sent through the solenoid, the core was drawn down into it, stretching the spring and carrying round a pointer attached to the axle of the pulley at the top of the apparatus. The spring is then still further stretched until the pointer comes back to its starting point, and then the current being interrupted, weights are attached to the core until the pointer returns to zero. In this way the direct pull of the coil is measured in grammes.

The first experiments were made with cylindrical cores of the same thickness but of various lengths, and the point at which the maximum attraction occurred was determined. The coil in all cases was the same, and 135 mm. long. With a core 30 mm. long the whole core was in the solenoid; 97 mm. long, 70 mm. in the solenoid, and 27 projecting above; 130 mm. long (nearly same length as solenoid), 88 mm. inside; 260 mm. long, 110 mm. inside; 390 mm. long, 130 mm. inside. It appears that so long as the core is shorter than half the length of the solenoid, the point of maximum attraction is reached when the lower end of the core has not yet reached half way into the solenoid. If the core is longer than half the length of the solenoid, the point of maximum attraction occurs when the core has entered further than half way, and for very long cores the lower end may be nearly at the bottom of the solenoid before the maximum attraction is reached. A direct comparison of the curves for cylindrical and conical cores showed that the point of maximum effect for the latter occurred when the lower end of the core was further into the solenoid than for the former. The paper is illustrated with numerous curves for various combinations of cylindrical and conical cores, built up to form one—one curve shows two points of maximum attraction. The reason why cylindrical cores are not suitable to regulate differential lamps can be deduced from the author's experiments, that the curves of the two cores will not be parallel, and consequently the point of equilibrium varies from one position to another.

UPPENBORN—DEPREZ'S PROPORTIONAL GALVANOMETER.

(Centralblatt für Elektrotechnik, Vol. 7, No. 7, 1885, p. 138.)

Five permanent steel magnets of horse-shoe shape are placed horizontally one above the other, a pole-piece being fitted on inside each leg of the compound magnet thus formed. The two pole-pieces are bored out to a true cylinder, inside which hangs an iron tube having a coil of wire wound on it lengthwise like a Siemens armature. The iron cylinder, being very close to the pole-pieces, the magnetic field is very powerful and very uniform. The iron cylinder and its coil of wire are supported by two stretched wires, one above

and the other below, which serve at the same time to make connection to the coil. A pointer and scale completes the instrument, which can be relied upon to give correct readings up to 120°.

F. KOHLRAUSCH—CONDUCTIVITY OF VERY DILUTE SOLUTIONS.

(*Centralblatt für Elektrotechnik, Vol.* 7, *No.* 9, 1885, *p.* 179.)

The degree of dilution to which the experiments were carried was very great, the quantity of salt contained in the solution being less than what is often found in distilled water.

The concentration of the solution was measured by its molecular coefficient m, that is, the quantity of salt dissolved in one litre expressed in grammes and divided by the molecular weight of the compound. The least value of m was 0·00001, which, in the case of common salt ($Na\,Cl = 58{\cdot}5$), would mean that 0·000585 gr. was contained in one litre of water, which is equivalent to $\frac{1}{17100}$ per cent.

The first experiments were made with neutral salts, and it was found that from $m = 0{\cdot}0001$ to $m = 0{\cdot}00001$ no great change took place in the conductivity, so that it appears that for this class of compounds a limiting value is reached. With salts giving acid or alkaline reactions the case is quite different. As the degree of concentration is increased from $m = 0{\cdot}00001$ to $m = 0{\cdot}001$ or $m = 0{\cdot}01$, the conductivity increases to a maximum value, from which it again falls away.

The author also deals with the question of the movement of the ions in electrolytes, and he proposes the hypothesis that in dilute solutions—that is, when the movement of both components of an electrolyte takes place in a medium which differs in composition very little from pure water—each component has a certain fixed motion independent of the other. This hypothesis he shows to be supported by the experiments which he has made.

ANON.—VON BEETZ'S GALVANOMETER FOR DEMONSTRATION TO LARGE AUDIENCES.

(*Elektrotechnische Zeitschrift, Vol.* 6, *No.* 6, 1885, *p.* 260.)

In the middle of a circular metal plate provided with levelling screws is a copper damper for the bell magnet, which is hung from a fibre. On the damper are mounted two small columns which carry the suspension tube. On one side of the suspended needle is a coil of thick wire, on the other side a coil of thin wire, the connections to which are carried through the base plate. Concentric with the axis of the needle is a large upright metal ring on which the divisions are marked in very large figures. To the suspension of the needle is fixed a straw carrying at its end a piece of white paper which moves over the upright scale. The whole is enclosed in a glass case.

OTHER ARTICLES.

(*Comptes Rendus, Vol.* 100.)

No. 17.—**DEPREZ**—Regulation of the Speed of Electro-motors.

(*L'Electricien, Vol.* 9.)

No. 106.—**E. HOSPITALIER**—Unity of Electrical Definitions, Symbols, etc.

No. 107.—*Anon.*—Bloch's Electrostatic Apparatus. *Anon.*—Bablon and Gallet's Electric Alarms. *Anon.*—Formula for Calculating Price of Lighting by Glow Lamps.

(*Journal de Physique, Vol.* 4.)

March.—**MASCART**—Damping Method for the Determination of the Ohm.

(*La Lumière Electrique, Vol.* 16.)

No. 17.—**C. C. SOULAGES**—Documents connected with the History of Electro-motors. **J. LUVINI**—Origin of Atmospheric Electricity.

No. 18.—**G. RICHARD**—Details of Dynamo Construction.

No. 19.—**G. RICHARD**—Idem. **F. UPPENBORN**—Kohlrausch's Measuring Instruments. **HURION**—Heating Effect of Electric Sparks.

No. 20.—**G. RICHARD**—Telpherage. **HURION**—Variations in Resistance of Bismuth in a Magnetic Field.

No. 21.—**G. RICHARD**—Integrating Dynamometers and Counters. **NEZERAUX**—Electric Accumulators. **KROUCHKOLL**—Polarisation of Capillary Tubes traversed by Liquids under High Pressures.

(*Annalen der Physik und Chemie, Vol.* 25.)

No. 1.—**L. LORENZ**—Determination of Resistance of Columns of Mercury in Electro-magnetic Units. **H. JAHN**—The Correctness of Joule's Law for Electrolytes. **H. KAYSER**—Photographs of Lightning.

(*Beiblätter, Vol.* 9.)

No. 2.—**VICENTINI**—Conductivity of Alcoholic Solutions of Chlorides. **KOLENKO**—Pyro-electricity of Quartz.

No. 3.—**PALMIERI**—New Researches on Volta Difference of Potential.

No. 4.—**HAGENBACH**—Speed of Electricity in Telegraph Wires. **EMO**—Resistances of most Important Wires at Various Temperatures. **KAZ**—Reflection of Light from Magnets.

No. 5.—**D. SOLOTAREFF**—Precautions in the Use of Mance's Method. **P. NOVIKOFF**—Grouping of Batteries to obtain most Advantageous Results. **STROUHAL and BARUS**—Effect of Tempering on Magnetic Properties of Steel.

(*Centralblatt für Elektrotechnik, Vol.* 7.)

No. 5.—*Anon.*—Elihu Thomson's Lightning Discharger for Dynamos.

No. 6.—**KESSLER**—Direct Measurement of Ampères, Volts, and Ohms with a Tangent Galvanometer. *Anon.*—Dynamo Machines for Use in the Laboratory. *Anon.*—An Electric Crane.

No. 7.—**FEIN**—Safety Cut-outs for Electric Light Leads.

(*Zeitschrift für Elektrotechnik, Vol* 3.)

Nos. 9 and *10.*—**FERRARIS**—Gaulard and Gibb's Generators. **LEWANDOWSKY**—Induction Coils for Medical Use.

JOURNAL

OF THE

SOCIETY OF

Telegraph-Engineers and Electricians.

Founded 1871. *Incorporated* 1883.

Vol. XIV.	1885.	No. 59.

The One Hundred and Forty-eighth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 12th, 1885—C. E. Spagnoletti, Esq., M. Inst. C.E., President, in the Chair.

The President: Before we commence proceedings this evening, perhaps you will allow me to express the pleasure that the Council feel in welcoming the members at this, the first meeting after the recess. We hope that the forthcoming session may be one of interest and information to us all, and that good papers may be presented which will produce full and instructive discussions. I am glad to see such a good attendance this evening, and I hope we may be able to keep up such numbers. I trust members will make an effort to attend these meetings, as there is nothing that tends more to the prosperity of the Society than large attendances; and there is nothing more calculated to give satisfaction to those gentlemen who bring papers before us, for our benefit and improvement, than a well-attended meeting and an appreciative audience.

The minutes of the Extraordinary Meeting held on June 11th, 1885, were read and confirmed.

The names of new candidates were announced and suspended.

The following transfer was announced from the class of Students to that of Associates:—

Frank Mercer.

Donations to the Library were announced as having been received during the recess from—The Astronomer Royal; J. Aylmer, Member; Shelford Bidwell, Member; Col. Sir Francis Bolton, Vice-President and Hon. Sec.; Latimer Clark, Past-President; W. Clauson-Thue; The Earl of Crawford and Balcarres, Member; E. Delaurier; Eden, Fisher, & Co.; Franklin Institute; Institution of Civil Engineers; Kew Observatory; Ch. Madsen, Foreign Member; Julius Maier, Associate; Ch. Mourlon, Foreign Member; H.M. Patent Office; W. H. Preece, F.R.S., Past-President; G. A. Rowell; Rev. F. J. Smith, B.A., Associate; Dr. D. Tommasi; R. von Fischer Treuenfeld, Member; University College; Major H. S. S. Watkin, Associate; Professor Dr. K. E. Zetzsche, Foreign Member.

The PRESIDENT: It is with great pleasure that I ask you to accord your thanks for these donations. Our library is one of the finest for electrical works in the world, and at almost every meeting we have a long list of accessions brought before us, thus adding to its value and importance.

The next subject this evening is the paper by General Webber, and I am sure you will be quite with me when I say that it is a matter of great pleasure and delight to us all to be able to welcome him again amongst us, and also to see him in the good health which he appears to enjoy, after the perils and dangers he has gone through in the recent long campaign in a most trying and unhealthy climate. It is a matter of congratulation to us to know that his services have been appreciated, and that they have been rewarded by the promotion which his well-known ability and energy so justly merit.

THE TELEGRAPHS OF THE NILE EXPEDITION.

By Major-General WEBBER, C.B., (Ret.) R.E., Past-President.

In 1882 I had the honour of giving this Society a description of the telegraphs which were used with the army that entered Egypt in that year. Those were the telegraphs of Lower Egypt; and the experience then gained was no doubt of use last year, when we had to deal with the telegraphs of Upper Egypt,

which may be called a separate system from those of the northern part of the country, although they all converge to the same central station at Cairo.

They are described on the diagram before you. [*See plan.*] You will see that four circuits are shown leaving the central office in Cairo. One is a through wire, which has always been used for the Soudan work; the second is a Morse wire, with three intermediate stations upon it, terminating at Assiout, but continued by a single-needle circuit to Assouan, with a large number of intermediate single needles upon it; the third is an omnibus wire for railway traffic, with about 26 single needles upon it; the fourth is a local circuit to Fayoum, with five single needles upon it.

On our arrival at Cairo, in September, 1884, we found there were constant complaints from the army stationed up the Nile, and from the General at Wady Halfa, of the inefficiency of the telegraph service.

The European superintendent sent forward by the General Commanding the Army of Occupation reached Assouan on the 6th September, from whence he forwarded daily reports of failures: for instance, I have notes of reports received from him on September 7th, 9th, 10th, 12th, 13th, 16th. On the 7th September he telegraphed to say, "Line broken down between here and Wady Halfa;" again, on the same day, "Assiout using peg cut us off from Cairo to-day from 3.45 to 4.45." On the 10th, when communication was restored at 2.15 p.m., he sent a message saying, "Communication has been totally interrupted between here and Cairo." On the 13th he reported, "As usual, Wady Halfa kept back his work again all last night, sending us his telegrams timed 6 p.m. yesterday at 7.30 a.m. this morning." On the 16th he wired, "Good telegraphists urgently required at Wady Halfa. Please send first-rate operators."

A report of delays between Assouan and Keneh, which reached me on the 21st of September, gave the average delay on 19 Arabic forwarded messages as 17 hours, on 5 ditto received 20 hours, and so on.

Telegraphists will naturally sympathise with us on account of the precarious state of such a line of communication with an

army, the business of which was rapidly increasing. The causes of these interruptions may be ascribed in a few words to occasional inundations of the Nile either when rising or falling, by which the embankment upon which the telegraph line stands is sometimes washed away, but is chiefly due to the decayed state of poles after years of arrears of maintenance. There they do not suffer from rain as we do in this country; indeed, Herodotus says that in Nubia a shower of rain was supposed to be the forerunner of the end of the world.

Absence of office organisation prevented the localisation of faults. The through wire was led into the Keneh office only; but faults could never be localised, although there were fifteen or sixteen single-needle offices on the second wire, which ran on the same poles.

The maintenance of the batteries at these offices was deplorable: the signals could never be read without dividing the circuit. In May, 1885, a return was received by me from the Inspector-General's own office, showing that none of those batteries had been renewed since the previous January, 1884—nearly seventeen months. The constant use of "earth" at all stations at the will of the clerks is a serious evil in such a country. That admirable form of instrument which is used by our Indian telegraphs, made by Messrs. Siemens, is also used by the Egyptian Administration. On it there is a peg switch by which the operator can put on "earth" on either side at will, or put himself through. I have myself seen a circuit "earthed" at an intermediate station in the middle of a message—an offence punishable with dismissal in any other country.

The absence of "code working" results in a condition of things which to telegraphists would create an impossibility of success. An examination into the control and working of the system makes one marvel how any results at all are obtained. If telegraphy was old enough, one would say it was of the last century, and that the Egyptian Administration had learnt its task in the infancy of the art, and had since lived amongst the absurdities and incongruities of telegraphic dreamland. There is no public opinion, it may be said, in Egypt which

could be brought to bear to produce a better state of things; and, moreover, being a despotic country, and the telegraphs having been first established there for the purposes of government, the whole of the circuits are led into the palace, where the Khedive has an office of his own, under his own operators, through whom he can control every message, both private and public, that passes over the lines. By the kindness and good sense of the present Khedive, however, the military wire was terminated at our office, and therefore our messages were beyond his control.

The Egyptian Administration, with the best intentions, was absolutely unequal to the coming strain. The Inspector-General, Mr. Floyer, was absent from Egypt in the Soudan, with the apparent intention of remaining. When we brought the line into full work it was found necessary to keep thirty first-class clerks on the main circuit alone. Only two clerks equal to such work were to be found in all the Egyptian staff, *i.e.*, sound-readers of English (particularly in cypher messages), accustomed to traffic discipline on a busy circuit with many stations. Temporary help had been lent by the army to the Telegraph Administration pending our arrival, and some soldiers of the line battalions, who were fair telegraphists, but not of the quality necessary, had been sent up country. It therefore became a most pressing need that the first section of the telegraph battalion which was arriving from England should be sent forward as fast as possible. It arrived in four detachments. Its strength was six officers and 118 non-commissioned officers and sappers, many of them telegraphists, either indoor or outdoor.

First detachment arrived in Egypt the 15th September, 1884. Captain Bennet, Lieutenants Tower, and Stuart, R.E., and sixty-eight non-commissioned officers, sappers, and drivers, with a field telegraph equipment of 20 miles of line and offices.

Second detachment arrived a week after. Captain Bagnold, R.E., with the balance of the equipment of 100 miles of line and ten offices.

Third detachment arrived the 29th September, 1885. Lieutenant Hill, R.E., and thirty-seven non-commissioned officers and sappers, with 160 miles of wire and insulators, without poles.

Fourth detachment arrived on the 8th October, 1885. Captain C. K. Wood, R.E., and thirteen non-commissioned officers and sappers (twelve of whom were Post Office Volunteers), and 176 miles of wire and insulators, without poles, with a double-current key speed duplex complete apparatus, two terminal stations with translation intermediate, contained in 218 cases, presented by the Post Office to the War Office.

The probabilities of the campaign were most likely as much known to the Director of Telegraphs as to anybody else at that moment. All he could look to was that the long line of communication should be efficiently worked; but he had also to consider that it was possible, if not probable, that an army in the field moving away from the Nile would require telegraphic communication. His duty therefore was, to push on all the resources of men and material at his disposal as fast as the means of transport would permit.

At Cairo, thanks to Mr. Le Mesurier, the Chairman of the Board of the Railway and Telegraph Administration, an excellent office was provided as the terminus for the army wire. There the Postal Telegraph clerks, under Serjeant Parish, worked night and day until quite lately.

At Assiout a diabeah by the bank of the Nile was provided for the office, as, owing to the extraordinary number of mosquitoes in a new building used at first, the telegraph clerks were more disabled during the first two or three days they were at work than many wounded men. At Keneh two clerks were left for the purpose of localising faults.

At Assouan an office was established in the town on the east bank of the river, the line being on the west.

The Egyptian Administration had constructed an overhead loop from one bank to the other about two miles above the town, *selecting* two unusually high bluffs close to the river which have

an average height of 250 feet; on these they placed masts of about 80 feet in height, thus affording an average height from the tops of the masts to the river of about 330 feet. Between these masts they endeavoured to draw up three wires so as not to touch an intermediate island. With a span of almost exactly a mile (it was within seven yards of a mile), and a sag of 330 feet, we can well understand that the wire very soon broke, and, in fact, was never strained up so as not to touch the island. Poles had to be placed on the island to support the wires, but it became so precarious a means of communication that we were glad when some armoured cable arrived which had been sent out from England. Capt. Bagnold laid this cable across the river, and built a new loop into the town, and it has, I believe, worked without a fault to the present day.

A line from Assouan to Shellal was opened on the railway by Major Spaight, R.E., and soon after, it became an urgent necessity to repair the main line from Assouan to Halfa, which is a wooden pole line, then in an extremely decrepit state. Many of the poles when examined were found to be so much honeycombed by the white ant that a blow from a stick would shatter them to pieces. I have counted on that part of the line upwards of six and seven joints in one span. Many insulators had to be replaced, and a great deal of other work had to be done. Lieutenant Hill was the officer in charge, having a diabeah for his men, while camels working on shore assisted in the work. The labour employed was taken compulsorily at each village, which is the usual practice followed in that country for repairs to the telegraph.

At the intermediate office at Korosko, a very important place on the line of communication, we found evidences of an attempt having been made at one time to span the river with a loop from the west to the east bank—an attempt which the absence of any height at the edge ought to have precluded.

On the arrival of the first detachment at Wady Halfa a temporary cable had to be laid across the river. This had to be done with lengths of the field telegraph ground cable, which, being in a position it was never intended for, where, by the friction of the current, it was rapidly injured, soon gave way, and had to be

frequently replaced, pending the arrival of the armoured cable. The last length of the ground cable was almost gone when the armoured cable arrived and was laid.

All the telegraphs south of Wady Halfa are called the Soudan telegraphs. They were commenced in the year 1863, in the time of Ismail Pacha, by Mr. H. I. Gisborne, late Chief Engineer of Egyptian Telegraphs. The extension southward from Keneh, up to which place the line had then been carried, was begun in 1865 by Salameh Bey, an old member of our Society, and was completed on wooden poles to Berber, *via* Abu Hamed, in 1868.* Meantime, in 1865, 1866, and 1867, the line to the interior from Suakim, *via* Kassala, to Berber was being constructed with wooden poles—brought from Tachwz, in European Turkey—by Messrs. Jacobson and Ford, and was completed during the Governorship of Giaffer Pacha to the latter place in 1868. †

So quickly were the wooden poles injured by the white ant that they had to be partially replaced by 6,000 Siemens's iron poles, sent up the Nile in 1867—a work which was completed *via* the direct desert route, Dugyet to Berber, in 1869. Thus Berber became a central station of the Soudan telegraph system, where all messages were transmitted, except when Khartoum was put through to Cairo.

During the rebellion which took place in 1883 the Souakim-Kassala-Berber line was destroyed by the Hadendowas. These men did not understand how the telegraph worked, but they knew that it could carry orders and news for the benefit of those whom they looked upon as oppressors, and their first feeling was to revenge themselves on the unfortunate Soudanese linemen who lived along the line. I have been told that these men had been soldiers in a regiment which had been decimated and disbanded by Ismail Pacha some years before, but that they turned out most

* Nubar Pacha was then Minister of Public Works and Director-General of Railways and Telegraphs.

† Further consignments of iron poles from Messrs. Siemens—10,000 in 1873, and 3,500 in 1875—with iron-hooded insulators and No. 8 wire, were sent out, and used in the extensions of the Soudan system further south under Gigler *Pacha*.

excellent telegraph linemen. Some of these poor fellows were tied to the poles by wire and burned alive; while in the case of others the Hadendowas had a very remarkable way of testing the telegraph: they thought that by cutting a wire and forcing the ends into the sides of a lineman's head, he would speak like a telegraph instrument, and would tell them what it was saying!

The staff of the Soudan telegraphs south of Wady Halfa was handed over *in toto* to the control of the army. It consisted of, clerks, line-guards, linemen, camel men, and gaffeirs, or villagers who acted as watchmen.

In October Lieutenant Stuart had been pushed forward beyond Wady Halfa by Dal to Dongola. He established *en route* the office at Dal, and arrived at Dongola the same day as Lord Wolseley's headquarters. South of Dongola the line had been more or less injured, and in places had been so completely destroyed that the wire was cut up and thrown into the river, the iron bases of the poles and the insulators were smashed, and the wrought-iron tops were hid or carried away. Temporary repairs were made to that line by a very intelligent line-guard of the name of Mohammed Bunduk, under instructions from the Mudir.

When Lieutenant-Colonel Colville, of the Intelligence Department, with his telegraph clerk—Sergeant O'Donel, of the Berkshire Regiment—reached Merawi, they were able to open an office at that place on the 25th October, the wire being supported and held above the ground by branches of trees, to which the wire was tied by rags or cords.

At that time (*i.e.*, November), headquarters being at Dongola, the main circuit was worked—

Cairo
Assiout
Assouan
Korosko
Wady Halfa
Dal
Dongola

with translation at Assiout, Assouan, and Wady Halfa.

On the east bank of the Nile, starting from Wady Halfa, there

is a railway called the Soudan Railway, upon which a line of telegraph had been constructed by Mr. Paoletti, of the Soudan Telegraphs, having stations at Gemai and Sarras. It was first intended for railway working traffic only, but military messages soon became so frequent on it that it was more used for that class of work. Extensions were made with field telegraph material from Sarras to other places on the same side of the river, namely, to Semneh, Mohrat, and Ambigole, and afterwards to a place called Akasha. By means of this alternative line serious delay, which might have been caused by one or two faults which occurred between Dal and Halfa, was prevented, messages being taken by messenger across the cataract at Dal and by road to Akasha in two or three hours.

The main line on the iron poles, as I have described, passes up the west bank of the river. Entirely carried on Siemens's iron poles, the wire was rarely faulty. I have great pleasure in testifying to their presence as having conduced more than any efforts of ours to the security of telegraphic communication through the Soudan portion of our line.* This line, of which Mr. Gisborne could give you a very graphic account, is 230 miles in length from Angash, opposite Halfa, to Dongola, the first 90 being through one of the most barren deserts and desolate countries that can be imagined. In no country that I have ever seen does such desolation exist. Black rocky mountains of igneous formation, with bright yellow sand. The rocks are so sharpened by the driving sand that if you sit down upon them the effect is very like that of sitting upon one of our garden walls covered with broken glass. In some places a sand-drift rises around the telegraph poles and conceals them. Then it is the habit of the native lineman to remove the insulator

* The military telegraph pole of the future, for all but the lightest equipment, will, I believe, be a Siemens patent iron tubular pole with slit joint and the Le Grand Sutcliffe base pile of wrought iron—length, 15 feet 6 inches, and total weight of 45 lbs.—which, when tested in a nearly horizontal position by fixing at one end and weighing at the other with a leverage of 13 feet 6 inches, carries a load of 110 lbs. It will be better without a lightning rod, and should be tapped to receive the bolt of a F.S. insulator. It should *have a light* bracket to carry another, when a second wire has to be run.

and stick a new pole-top on the standing one, replacing the insulator, and so raising the wire. Two or three years afterwards the same process has to be repeated; and I was shown one place where four poles were said to stand one on the top of the other.

Dal office was a lonely place for two clerks to remain in for six months in a mere Indian mountain tent, entirely alone, with a wide cataract between them and all assistance. There they remained, and I do not think they ever saw an Arab the whole time, except the usual passing caravans, and were certainly never molested. That office was moved twice, and eventually was placed on an island in the centre of the cataract.

As one marched along, inspecting the line and visiting the native staff, very curious things were encountered. One day I saw a telegraph ladder lying in a little native cemetery, which had no enclosure, and was merely marked by a few upright stones and a number of pebbles bordering the graves to mark where they were. I asked the lineman why it was there, and he said it was where the ladder was always kept. I then found a coil of wire, and a further search revealed the presence of a draw-vice. The man then said that the cemetery was the only safe place in which to keep telegraph stores, whence, on account of the Spirits, nothing was stolen. It crossed my mind as to what my old friend Mr. Winter, the Controller of Postal Telegraph Stores, would say if it became necessary to move his stores from Gloucester Road to Kensal Green. I asked the lineman if an English gold sovereign would be equally respected, but he seemed to think it had never been tried.

An office which became of very great use was established at Abu Fatmeh by Lieutenant Tower, who was also employed on insulation and repairs to the line south of Dongola. The office in the town of that name was certainly the most palatial of its kind in that country. The walls were of mud, 10 feet high; the floor was mud; and the table was actually of wrought wood, and the waiting room had a divan with cushions on it.

At Debbeh an office had been established by Major Kitchener. That place was well known for its defence, in the summer of 1884, against an attack of between 3,000 and 4,000 of the emissaries of

the Madhi. Bunduk, the line-guard, assured me that he was alone in the office, which he defended through the window with his Burden rifle, that he fired off 263 rounds, and killed 67 men, whose bodies, lying below the window, he afterwards counted.

At Korti the office was first constructed with straw walls, a material favoured in that country for cool residence, but also very favourable for the admission of dust and wind; and any one who spent a day or a night (and our telegraph clerks spent many days and many nights) in that office would have felt it extremely uncomfortable. The wind was always blowing, the dust was always driving through the walls, the candles at night were always flickering, paper blowing about, and everything that was antagonistic to good telegraphy was in force.

The office at Merawi, which I have already referred to, was in a native building—the old telegraph office—on the left bank of the river, opposite the town.

In January an extension was made to Hamdab on poles as crooked as those which I have already mentioned, cut from the mimosa and other trees in the neighbourhood. It was put up by Lieutenant Stuart, but was not worked after General Earle's column had started up the third cataract. This extension was about 19 miles in length, and followed the route of the iron pole line which the rebels had destroyed as far as Dugyet, where the latter branched across the desert by Beni Sani to Berber. [*See map.*]

In the meantime our men were by degrees moving up the Nile to be ready for eventualities. Captain Bagnold started from Sarras with a detachment in whalers, exactly under the same conditions that were obligatory on all infantry detachments. Each boat started heavily laden with its equipment and stores, and had to arrive at Korti with its full complement of rations untouched. This obviously left no room for telegraph material, some of which was however brought up in native boats, which often proved unequal to the task as the water in the river became lower.

I have said that Mr. Gisborne would be able to give you a *graphic* description of that country, but I had the advantage of

him in one thing which I occasionally saw when I came down from the desert to halt on the banks of the river. It was a delightful sight to see the little fleets of white-sailed boats either running before the wind, or with their crews pulling hard at their oars, or being towed along the bank; to hear the bugles ring out in the early morning from every resting-place, perhaps for miles down the river, when the men were being roused to start again on their long journey; and it was a wonderful thing to realise that so much boatmanship and so much energy was being displayed by the ten men in each, perhaps not one of whom had ever before handled an oar, steered a boat, or pulled a tow-rope.

The transport difficulties south of Wady Halfa prevented the mass of our equipment being brought forward. No provision had been made for telegraph transport. Camels were scarce; intelligent care of them was wanting; they were dying much faster than any one expected; and so the forwarding of poles and wire was really a difficulty which nothing but the power to arrange in advance could have overcome. I will give you an example of length of time in transit. The seventy cases, No. 2 station, which eventually arrived at Korti, and which were forwarded by every possible express means, took from the 15th October till the 24th December to reach Korti. Only by using every opportunity, and sacrificing the small personal baggage and mess stores which senior officers were allowed to take, did I succeed in bringing up on a few discarded camels the heliograph equipment and the bare necessaries to work and maintain the telegraph offices and to insulate the line. The instruments sent to us by the Post Office were intended, as I have said, for double-current duplex working, but we had to arrange to work them single-current simplex. The reasons were, that on account of the very faulty state of the line, ordinary translation had to be early resorted to at Assiout and Assouan. By the last day of the year, when the circuit was put through to Korti, the work was too heavy to make any change. I made several appointments with Captain Wood, who was at Assouan at the time (while I was at Korti), to come into circuit with

the new instruments and make a trial, but on each occasion we were prevented by press of work or by serious faults on the line. The heavy local work on a circuit with eight intermediate stations also made it difficult to use any system which required the line reserved for terminal working. We wished to have worked between terminals at night for a few hours to get our heavy work through, but the fact that the line between Assiout and Cairo was never in a state of insulation to allow it prevented us. But, if it was not worked in its integrity, it gave us most valuable assistance with the standard relays, pony sounders, and six-block agglomerate cells. Without this special equipment, so readily and promptly contributed by the Post Office, we could not have achieved the results I am describing. The only translation that was really used at the intermediate stations is shown in the diagram. You will see that, when divided, both circuits work either simultaneously or independently [*describing the diagram*]. A relay and sounder are always in circuit at translating stations, so that the work can be watched. Its disadvantage is that there are four relays, and the line battery is earthed at the centre. [*The course of the incoming current, its action upon the relays, and its translation, were traced through the diagram.*] With heavy traffic on a defective line much delay may be saved by the intermediate watching, and the intermediate stations can come at once into work, and transmit when required.

Captain Wood made some interesting tests of the line in Upper Egypt, taken both morning and evening, from the 1st of December till the 31st of March. The I.R. and C.R. taken in that period were on two sections—one between Cairo and Assiout, 230 miles, partly of No. 6 and partly of No. 8 wire; and one between Assiout and Assouan, 310 miles, of No. 8 wire. In the first section the insulation resistance was below 5,000 ohms on 38 mornings and 19 evenings, on nearly half of which it was below 1,000 ohms. The conductivity resistance, however, steadily rose from 1,350 to 3,400 ohms, showing that the efforts to improve the insulation had succeeded to a certain extent. In the second section the I.R. was below 5,000

ohms on 10 mornings and 5 evenings; the C.R. fell steadily from 5,100 to 3,100 ohms—probably due to the cutting out of dry joints. On those occasions the current received from 28 cells at Cairo, when the I.R. = 1,500, and the calculated C.R. = 3,450, was ·037 milleampères per cell, or a total of 1·016. When the I.R. = 1,000, and the continuity the same as before, the current received was 1·736 milleampères; when it was 2,000, it was 2·632 milliampères; when it was 5,000, it was 3·64 milleampères. In the second section, when the I.R. = 500 ohms, and the calculated C.R. = 4,650 ohms, the current received at Assiout from 40 cells at Assouan was 1 milleampère; with 1,000 insulation resistance it was 1·68; with 2,000 it was 2·64; and with 5,000 it was 4. With the most delicate adjustment of a Post Office standard relay, the presence of a current of 2 milleampères can barely be detected, with an improved Siemens relay much more current is required, so that a working current of 16 to 20 milleampères at least was necessary. These were the results of the daily tests of the line. But, in addition, all actual faults occurring and causing interruption to through traffic were reported immediately by wire to the Telegraph Administration, and daily in detail in writing, according to the home custom. When the time of these interruptions, as taken from all the office diaries, was added together, it amounted to 40 days of 24 hours in six months!

The Post Office pattern block agglomerate Leclanché batteries, with an E.M.F. of 1·5 and R. of ·6, were our principal stand-by. Besides, we had the field service Leclanché batteries of the equipment, which suffered a good deal through the dryness of the climate and jolting in transport on camel-back. We also had to use Minotti batteries which we found in the country, as well as ordinary Daniell cells, of which we found some in the Soudan. The military Morse recorder now in use is an improvement on that called "Morse Recorder, Universal Pattern, No. I.," which I showed here in 1882. It has superseded the latter, and is marked "No. II." Its advantages are very great, and it well deserves, I think, the name of "Universal Pattern." The winding of the coils, which are 150 ohms each, is side by side, commencing in the centre of each

bobbin, so as to avoid inside ends, and offering the advantage that whichever way the current passes, the same result on the electro-magnet is produced; they can be joined either in series or parallel, and their resistance in series is 300 ohms, and in parallel 75—conditions suitable to direct and local working respectively. These instruments can be worked with a closed circuit by moving a switch, and can also be used as a relay, besides being differentially wound to work duplex.

The vibrator arranged by Captain Cardew, and constructed on the same principle as the vibrator of Elisha Gray, shown here by me in 1878, was connected as a shunt to a circuit, with a Bell magneto-telephone as a receiver. On depressing the key, intermittent currents are sent to line, which can be heard as a musical note in the telephone, or telephones, in circuit, even when the intervening conductor has so high a continuity resistance and so low an insulation resistance that no ordinary working current would be perceptible. The reception of this buzzing sound by ear is no doubt fatiguing to those accustomed to read by the sharp click of sounders, but it was resorted to from time to time between Assiout and Cairo, and it was only when that altogether failed that Lord Wolseley was obliged to order special engines to take important messages. Once, between Dongola and Debbeh, when an earth on the line prevented ordinary working, the vibrating sounder worked with good results for several hours. Mr. Paoletti started on his camel to find the fault, and took with him a telephone and some "leading" wire. Whenever he came to a place where earth could be obtained, he put a "lead" from the line wire through the telephone to earth, and thus detected the buzzing sound. He travelled till dark, repeating the test, still hearing the same sound. Next morning when he awoke, on again testing with the telephone, he heard the clicking of the sounder, and, knowing that ordinary working was resumed, returned on his ride of 40 miles to Dongola.

Means for signalling formed part of the field telegraph equipment sent out for the Nile Expedition. It was intended for heliographic communication with Sir Herbert Stewart's

column across the desert, but it was not our fate to be allowed to use it, although with some labour it was duly transported to Korti in good time. With the column itself while on the march signalling officers and men communicated between the front and rear of the column, also between the outposts and the General, and rendered good service.

Heliographing from Korti to Gakdul was ordered in February, but countermanded on account of the necessity of using for more pressing needs the camels that would have been required for sending out supplies to the signalling stations. Eventually, during the retreat of the column from Gakdul, the heliograph was used by the rearguard for communicating with the main body.

Communication by heliograph is a means which is most suitable in the Soudan, in spite of the mirage. The country consists of great plains, interspersed with isolated hills, and here and there a range with high table lands; and the only real difficulty, if stations are established far from the river, is the amount of transport required to feed them and to supply them with water. I do not believe that isolated stations in the Bayuda Desert would have run any risk of being molested by any vagrant parties of Arabs which might have approached them.

Telegraphy and signalling, to the mind of a telegraphist, are one art; they are both means to the same end, and although they are handmaidens of the same science, they have been—I was going to say divorced—separated by the regulations of the British Army. Still I think there will be a union of them one day. Common sense seems to point in that direction, and I am quite sure that all the officers who have been employed in both feel very strongly that but one organisation and one direction is necessary for the successful application of these means.

The use of our telegraph lines in the work of the lines of communication was very great. Sir Evelyn Wood, the General of Communications, sent upwards of 3,000 telegrams in six months, all written by his own hand, and most of them of more than fifty words.

Of the innumerable cases in which time was saved by its use

it would be impossible to attempt to give an idea. For instance, it saved time in the following way in an expedition over so long a line as you see on the map. At every place beyond Assiout rations were scarce—very much so beyond Halfa. It was of great importance that the stores should not be used up by stationary or waiting bodies of troops. By means of the telegraph the troops were detained until the last moment where food was plenty, and then sent forward when they were actually wanted. Any other means of communication, by post or messenger, would not have achieved the same end; and if one calculates the time that must have been lost without the telegraph, it seems quite possible that without that means of sending orders the expedition would have been impossible altogether, at any rate in any reasonable time, and its cost to the nation enormously increased. Orders for the movement of individual detachments and stores of every kind were flying over the line at every hour of the day and night.

I think it may be safely said that the arrival of the expedition at Korti, the advanced base, was due to three energies; and, without going back to the prime causes of energy, that brains—was the first; the north wind—was the second—it blew four days out of five, and none of the coal for the steamers could have been brought up except for the north wind; and, it is not too bold I hope for me to say, electricity—was the third. It is very difficult to say what was the measure of time saved by the telegraphs; I do not think it could be mathematically calculated out. It would be easier to calculate the foot-pound result of the wind on the sails of the diabeahs and sail-boats. The Generals and Staff, who were using it every hour of the day and night for six months, can best testify to its value; but, as with our postal telegraphs at home, those who used it took it all as a matter of course, and only when there was cause of complaint was this silent machinery ever heard of or its existence noted. That complaints were very rare, and that its value received no public recognition, is perhaps the highest praise it could have.

The most trustworthy history of that campaign, and the *evidence of* all the phases of its progress, are to be read in those

telegrams. The telegraph, of course, was used not only by the General Commanding-in-Chief for his cypher messages, which were of the greatest importance, but also for press and private messages, and, as has been said, for all the work of daily seeking and pressing forward everything that was wanted by every department of the army. On one night 17,000 words were forwarded to London. The General's cypher messages of course required great accuracy, and, being written in several cyphers, required considerable experience. We had no complaint of inaccuracies, but, as regards the messages to Europe, I must not forget that equal credit is due to the Eastern Telegraph Company for their share, which was the work between Cairo and London.

Ah! sir, it was a stirring time, I can tell you, when those messages were handed in, and we worked all night often till six in the morning, and we knew that those accounts, which were thrilling us quite as much as they interested you at home, would be in your daily papers that very morning. I daresay you will all recollect them. They gave a great deal of work, and our men did their work well.

All press and private messages had to pass under a censorship, so that they should not contain anything that could be made use of to the detriment of the army. This would have been of little use but for the secrecy and discretion of the whole telegraph staff. The Director of Telegraphs was made personally responsible for the success of the arrangements which secured it. If it had been in the days of Cromwell, he would have run the risk of losing his head. As you see him here safe and sound, you must assume that there was nothing of that kind that went wrong.

A large amount of Arabic work necessarily passed over the line, and precautions had to be taken against its being used for treasonable purposes. Without special precautions and constant watchfulness there was nothing at all to hinder two Arabic clerks, when they were using the circuit, talking to one another, and betraying any secret or conveying any information; in fact, we know that emissaries of the Mahdi did try to send telegrams, but we also know that they did not reach the people for whom they were destined.

At vital moments, when it was necessary that our General's message should be in the hands of the Secretary of State for War long before any other that might contain reference to the same subject, it was obligatory to make it absolutely impossible for any miscarriage to occur. Figuratively speaking, the telegraph forwarding key was put under lock and key, and I kept the key in my pocket. Awaiting the preparation of the message, all work to and from Korti was stopped. Preparatory C Q's were sent calling the officers and clerks-in-charge at each station to stand at "attention;" they were then told, when the message was ready, to stand clear, and to watch the working of the translators from a distance. The key at Korti was set to work, and until the whole message had been received in Cairo and repeated, and had been handed over to the Eastern Telegraph Company, no interruption or interference was allowed to take place.

Telegraph regulations, which had always before been wanted for the service of the army telegraphs, had been drawn up in 1883 by a committee of which I was President, and brought into use just before the starting of the expedition. That little book of regulations was of very great use. If it had not been in existence, most of its regulations would have had to have been telegraphed; as it was, if attention had to be drawn to any subject, all that was necessary was for the Direction to refer to them. Only 58 C Q's were sent during the whole time, but it was found very necessary to control, direct, and guide the business by conversations on the wire. Most of those which the Direction held were carried on at night with the various persons responsible. Their total amounted to over 50,000 words, of a tariff value of over £1,300. The personal anxiety and strain of ever being mentally present, and of having to realise daily, if not hourly, what was going on in working and maintaining that 1,150 miles of precarious telegraphs, I will not deny, was very great, and sometimes, when unnecessary difficulties harassed one, and particularly when health failed, almost exceeded the limits of endurance. But there was one good thing to stand by, the value of which every telegraph manager will understand, and that was the loyalty of the staff under me.

We were obliged to adopt the tariff in use in Egypt and the Soudan, for reasons which it would be too long to explain here. It is a regional tariff, and you can imagine the complication of it when I tell you that the first region is Lower Egypt; the second is Cairo to Assiout, both places included; the third, south of Assiout to Esneh; the fourth, south of Esneh to Wady Halfa; the fifth, Wady Halfa to Dongola; the sixth, south of Dongola to Berber; and the seventh, beyond Berber. Within one region the tariff is ten piastres for five words, or 1s. 0½d.; between regions it is double; and for each intermediate region a similar charge is added. Amongst the multifarious duties of our clerks, they had to receive payment for messages in all sorts of money. They found Marie Theresa dollars in use in one place, Colonat dollars in another, besides French and English money; and, again, all kinds had to be brought to account in piastres, and the piastre value sometimes changed. To get halfpenny stamps we cut our penny stamps in two. I do not know whether anybody has resorted to that before.

Warrants, something on the principle of the Cheque Bank system, were kept at all offices, so that persons authorised to draw cheques, and, who had credit at the War Office, were enabled to pay for their telegrams without carrying money about with them.

The Eastern Telegraph Company conferred a great boon on the army. They allowed what were called "privileged messages" to pass over their line free in cases of, sickness, death, domestic affliction, wounds, and such like. Mr. Schnitzler, Reuter's agent at Cairo, compiled daily a bulletin of news, sometimes of very considerable length, and presented it gratuitously to our office. This bulletin was sent by telegraph to every military station you see on the map, and a copy was suspended within half an hour outside the telegraph offices; and if you had seen the British soldiers gathered round them —sometimes one fellow reading out to the rest—you would never say that they were not pleased and delighted to get news of home.

I mentioned the Arabic messages. There was one gentleman (who I hope is here, but I cannot at the moment see him)—Mr. William MacCollough—whose services were placed at the disposal

of the army, to whom is due the honour of having had the charge of a most important duty. He was stationed at a point on the line where he had to watch every Arabic message that passed. During six months, any time at night and day, he was liable to be called upon to attend. He thus watched upwards of 5,000 messages, reading, of course, everything that was necessary, and detecting what should not go through. He was the only person, I think I may safely say in all Egypt, if not in the habitable globe, who, being able to read freely the Arabic Morse signals by ear, could have been entrusted with so responsible a duty. It was performed in a way which could only have been obtained from a man of Mr. MacCollough's high character and stamp. I very much fear that this service, so essential to the safety of the army, and so exceptionally confidential in its nature, will be passed over unnoticed, and, as is too often the case, fade from memory in competition with those of a more noisy kind; indeed, it is more than possible that faithful service to the British Army in connection with the telegraphs has had the effect of damaging the career of any one in the Egyptian service.

We had a clearing house—for the first time that a clearing house on an extensive scale has been employed in the army. Mr. A. Oatway, of the Post Office Telegraphs, was sent out to take charge of it, and I cannot lose this opportunity of expressing my gratification and thanks to him for the way in which the work was done. He was in charge not only of the work of clearing messages, but of keeping accounts of the most complicated kind, besides communicating with all the administrations, the newspapers, and so forth. He also had to keep the records of my office for me. Now, a Director of army telegraphs cannot travel with his office; his camel carries very little office fittings, I assure you; so that it was necessary, during the expedition, for him to have his office 1,000 miles away from him at Cairo, and that was conducted by Mr. Oatway, to whom everything of importance was conveyed by telegraph, and all documents sent afterwards by post.

The value of all telegrams had to be debited to two accounts—one the Egyptian, the other the Soudan, Government; to a general

account everything had to be brought, and then a subdivision was made between these two Administrations. There were further subdivisions (I tell you this so that those who were engaged in the work may feel they have not been forgotten); for instance, Her Majesty's Government, the Egyptian Government civil and military, the Soudan Administration, the Mudiriate of Dongola, the Khedival account, press accounts with each newspaper, besides private message and warrant accounts.

Now, as to numbers, though I am afraid statistics are always somewhat dry. I will give you one or two, which I hope will not tire you.

We cleared in six months over 43,000 messages, of the tariff value of over £23,000; besides about 11,000 D.S. and S.G. messages of 216,680 words, of the tariff value of over £5,000; and during that time (I hope some representative of the Eastern Company is here to hear it) we collected for the Eastern Telegraph Company over £10,000. That will, I hope, compensate them for the heavy loss on the lowering of their tariff between this and Egypt, which I, for one, considered unnecessary on the grounds upon which it was apparently made; for I do not believe, with my knowledge of the working of telegraphs in Turkey, that a lower tariff by that route would have had a chance of competing with the cable.

In the month of January, 1885, it had become evident to Lord Wolseley that the conditions of maintenance of the lines in Upper Egypt, upon which the communications of the army depended, had become too precarious to permit of the continuance of the arrangement which had been perforce accepted in the September previous. The utility and necessity of the telegraph as being vital to the service of the army had by that time asserted itself. It was therefore decided to move the Egyptian Government to transfer that part of the system to military charge; and in March I was despatched from Korti to carry out this transfer, and to make the necessary financial arrangements connected therewith. The journey was accomplished to Cairo, in spite of shipwreck (and consequent loss of many worldly goods), contrary winds, sand banks, and the doctor's prohibitions to travel, in 21 days.

Owing to several unusual difficulties, it took till the 1st of May to complete the change by which the system of Upper Egypt that you see on the map, with all its lines, offices, material, staff, and collection of revenue, was brought under military direction, including about 500 native indoor and outdoor *employés*, under Mr. Charles Clarke, C.M.G., who had for some time been stationed at Assiout, and who had been endeavouring to the best of his power to improve the telegraph service of that part of the country, and, in so doing, to prove his acknowledgment of the distinction conferred on him in 1882.

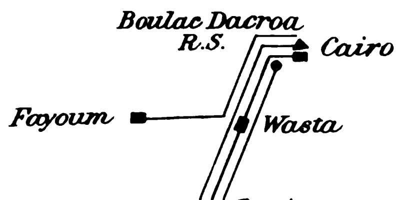
Unfortunately, this change of things came too late to afford the results for the army which would have certainly followed had the operations been continued. At any rate, it was satisfactory to know (when eyesight failed later on) that the whole machine was under control, that all was capable of being made to work smoothly, and that the General and the army could have been served in a way which, so far as was achieved, in some respects meets no competition in the annals of the use of telegraphy in war.

Sir, if in endeavouring to present to you and my hearers some account of the telegraph work of the Nile Expedition, my narrative has been prolonged beyond the limits of your patience, I trust I shall be pardoned by yourself and the audience, who probably can now understand the difficulty of condensing such a subject within the ordinary limits of a lecture. In judging of our work, pray recall that our occupation shunted us off the main line, the terminus of which is the goal of every true soldier's hopes and ambitions, namely, the presence of the enemy; and that when all further advance of the byway to honour which we had to follow was denied to us, then, indeed, came our hour of trial. It will be hard for me to forget that feeling; but there is a great satisfaction left, if, by what I have told you of the culminating work of my telegraph career, my narrative has done justice to a gallant band of officers and men, who worked, I believe, as hard and with as good results as their comrades whose work received the highest praise.

ARMY TELEGRAPHS.

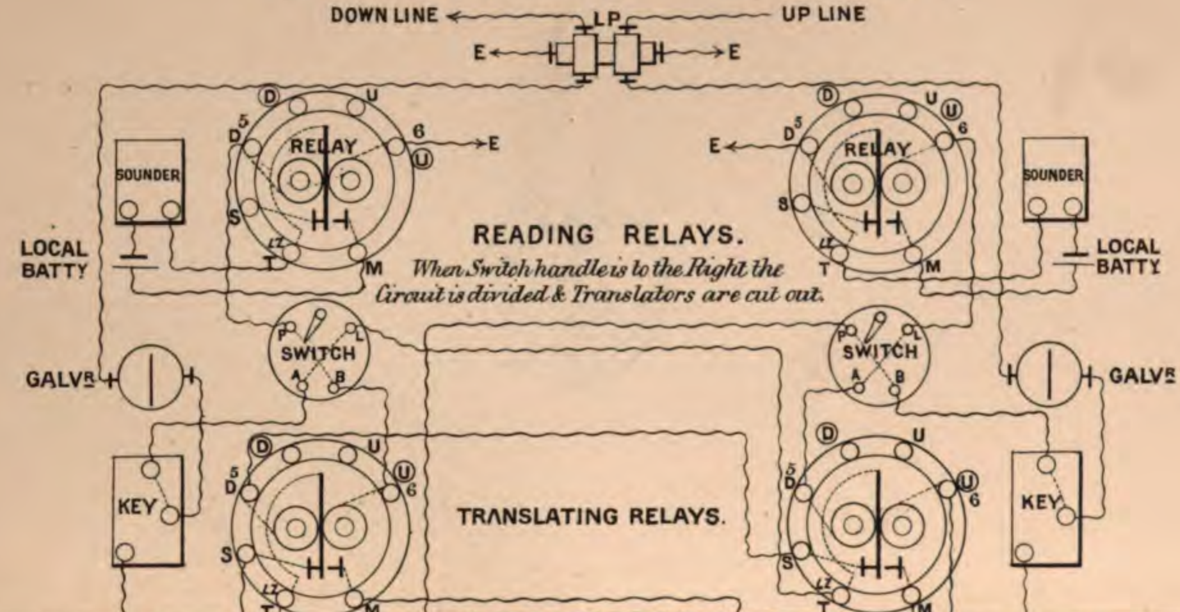
NILE EXPEDITION, 1884–5.

N.B.—*The circuit arrangements shown below are those which were in existence in May*, 1885.



		Miles.
Cairo to Assiout,	,,	229.

AUTOMATIC TRANSLATION.
AT ASSIOUT, ASSOUAN & WADY HALFA.



The PRESIDENT: Gentlemen, we will now discuss the very interesting paper which General Webber has been good enough to give us. He has shown us that the position of a telegraphist at the seat of war is anything but a comfortable or pleasant one, and that he is surrounded by as many dangers as those who have to go to the front; and the position appears to be the more trying by the deprivation of the stimulus and support of companionship, and by their being left comparatively unprotected, with so little chance of successfully defending themselves against the attack of any small bodies of the enemy, instances of whose revolting barbarity General Webber has given us to-night.

Col. R. HARRISON, R.E.: Mr. President, I was not prepared when I entered this room to make any remarks, but as I have been called upon by you to say a word or two I will not decline to do so. With reference to the very valuable lecture which we have received from General Webber to-night, I will only say this one thing, that I am glad to bear what small testimony I can to the exceedingly good and valuable work that was done, not only in the Nile Expedition, but in all the expeditions of modern times, by the military telegraph-engineers. It seems to me, if I may say so, that the reasons which have enabled them to do such good work in modern expeditions are three, viz.: they are exceedingly well organised; they have the very best stores that the country can produce; and there is an intimate connection between them and the great civil telegraph organisations of this country.

The PRESIDENT: We are fortunate in having so many military gentlemen here to-night. I will ask Captain Beresford to favour us with his remarks.

Capt. BERESFORD, R.E.: Mr. President and gentlemen—I will not detain you very long, but I should like to say a few words about the campaign in the Eastern Soudan—that is, the expedition that started from Souakim, with Berber as its object. We did not get very far on our road, as you all know; in fact, we did not go more than 20 miles up country, and so we had not the experience that General Webber had, but what we did brought out a good many points and gave experience for future wars. There are several points I should like to draw attention to.

Perhaps some of you are not aware that we had with us fifteen of the best clerks the Post Office could send—civilian clerks, volunteers, belonging to the 24th Middlesex. To show the necessity for having the very best clerks on war service I will just give a few statistics. We had four principal offices out in the Eastern Soudan—Souakim, Quarantine Island, Handub, and the Headquarter camp. Quarantine Island was the centre of all the telegraphic communication, and it was in work for 81 days. The average number of messages per day was 135; the average number of words per message 25. During the month of May we had a daily average of 214 messages at Quarantine Island; on the 15th May, the day that orders were given to embark and go home, 811 messages were dealt with, principally on two out of five instruments in the office. That means a very large traffic, and I lately compared it with that of some of the offices in England. I will take Exeter. The other day Exeter, with 36 instruments, had 2,470 messages; that is, the traffic on Quarantine Island on the 15th May had more than twice as great an average as the traffic at Exeter on 6th November. That shows you what the traffic may be, and how we must have the best clerks to deal with it. The next point I should like to draw your attention to is the use of No. 14 hard drawn-copper wire. It was an experiment taking that material out. Some people believed it was suitable, while others did not. We heard that it answered well on the Nile and in South Africa, but ours was a different case. We had only 20 miles of line, and that line was broken down, I may say, every day by camels, and, at one period, every night by the Hadendowas, and in the course of a few weeks it became mere patchwork. Those who work with No. 14 copper wire know that the least scratch on it will probably cause it to break. It is very difficult to manipulate, and very difficult to repair. When we went out first the manufacturer of this wire told us on no account to reel it on our military drums; it would not stand that, we could not work it. But we did reel it on the military drums, and it came out all right, and we reeled it up again when finished; at the same time it was very difficult to manipulate during constant repairs, and $\frac{3}{18}$ strand is in every way better for military

service, in my opinion. We had no complete telephones except what we borrowed from the navy and others, and they were very much wanted. One point that will, I am sure, interest you, is that it was the first time in any European army that the telegraph has been in the fighting line. Major Turner and Lieutenant Lindsay, both officers trained under the Engineer-in-Chief of the Post Office, brought the telegraph line into the fight at "To Frek" (or McNeill's zereba). Arriving there about eleven o'clock in the morning, it worked all day to Souakim without break, but at about seven o'clock that night the cable was cut. That zereba was attacked by the enemy, and the telegraph detachment took part in the defence. I think that has not been generally known—certainly not published in the papers—but it was the first time in any European army that the telegraph has taken part in a fight. I hope it will not be the last.

The PRESIDENT: Perhaps Captain Hammill, R.N., who knows the country very well, will give us some remarks.

Capt. HAMMILL, R.N.: Mr. President and gentlemen,—I felt very much honoured in receiving your invitation to attend the meeting to-night, but I had no idea that I should be called upon to make any remarks to the meeting. At the same time I was glad to be able to attend to hear General Webber's lecture on the telegraphs of the Nile Expedition, and I am convinced of the immense assistance given by this branch of the service to that expedition. Colonel Harrison, in his remarks, as reasons for the success of the military telegraph, mentioned three principal subjects; but I think he omitted one which is perhaps the most important of all, and that is, the zeal and diligence displayed by the officers and men of the telegraph companies, for without that I feel perfectly sure that not one tithe of the work which was carried out on the river could possibly have been accomplished. The organisation, as far as one could see, on the river—and I had many opportunities of observing it—and the means supplied, were perfectly efficient in every way. I think that all those who took part in the expedition thoroughly appreciated the immense service rendered by the officers and men of the telegraph staff on the Nile.

The PRESIDENT: Mr. Oatway will perhaps give us his remarks.

Mr. A. OATWAY: As you, Mr. President, have been kind enough to call upon me for a few remarks, I will, with your permission, give some additional figures; and as these figures include the receipts, and, to some extent, the expenditure, from the commencement to the end of the expedition—*i.e.*, from the 1st October, 1884, to the 31st July, 1885—they will necessarily be of interest, and perhaps of some value. I may mention that the accounts for the "Frontier Field Force" commence from the 1st August.

Taking the paid work first—that is, the cash value taken over the various army telegraph counters—I find the value of military telegraph stamps affixed to such messages, £2,814 18s. 10d., and the money collected at Cairo on the telegraph warrants drawn upon agents there, £450 15s. 8d. The Eastern Telegraph Company have paid us £622 3s. 5d. for messages handed over by them for our stations in Upper Egypt and the Soudan, so that the revenue paid to army paymasters in Egypt amounts to £3,887 17s. 11d. The sum due to the army telegraphs from the Telegraph Administration of the country, for similar work done, is £976 10s. 3d. These figures do not include any press work. The total value of the private and press work, including both the Eastern and Administration accounts, amounts to £18,363 11s. 9d., and, out of this sum, £12,268 15s. 6d. represents the value of the press messages despatched by the various newspaper correspondents. The telegraph warrants drawn upon Messrs. Cox & Co., army and navy agents, by various officers, amount to £1,118 5s. 9d., and those drawn upon the Chief Paymaster in Egypt to £61 7s. 3d. We have paid to the Eastern Telegraph Company, for messages handed to them, the large sum of £25,786 6s. 11d., and to the Telegraph Administration of the country, for messages in Lower Egypt, £847 12s. 7d. The total number of messages sent over the army telegraph lines during the ten months was 125,936, and their value (including the O.H.M.S. messages, but excluding the service messages sent by the Director of Army Telegraphs to his officers, or from his officers to those serving under them as inspectors, &c.) is £37,728 16s. 8d. This represents the value of *the work* in Upper Egypt and the Soudan. Taking this sum of

£37,728 16s. 8d., also the £25,786 6s. 11d. paid to the Eastern Telegraph Company, and the £847 12s. 7d. paid to the Egyptian Administration, we get the respectable sum of £64,362 16s. 2d. It is very gratifying to me to be able to state that, notwithstanding the tremendous pressure after each engagement (we have on several occasions received at Cairo during one night 16,000 or 17,000 words, as stated by General Webber, many of the press messages reaching the enormous sum of £400 or £500), I have only received one complaint of delay from the press, which, on investigation, I found rested with the Eastern Telegraph Company, and was satisfactorily accounted for by them.

I hope, Mr. President and gentlemen, that I have not wearied you with these figures, and, in conclusion, I beg to thank General Webber for the kind remarks he has made concerning myself, and also beg to assure him that nothing would afford me greater pleasure than to accompany him as his assistant in a similar expedition.

The President: Major Jelf is here, and as he has just returned from his charge in Bechuanaland, perhaps he will kindly favour us with a few remarks upon this most interesting subject of military telegraphs, and give us the advantage of his practical experience.

Major R. H. Jelf, R.E.: Mr. President and gentlemen,—I came here with great pleasure to hear what General Webber had to tell us about the operations on the Nile, without the least idea that our efforts in South Africa would be likely to come before the notice of the meeting.

The chief remark that I have to make upon the subject of the telegraphs in South Africa is that we were most wonderfully fortunate in their erection: the elements were favourable, and everything seemed to go as smoothly as possible; there were comparatively few difficulties or disadvantages to speak of. The chief distinction between the telegraphs General Webber has described and those which I had the honour to be connected with in South Africa was in the fact that ours was essentially a field telegraph; we carried nothing but field telegraph stores. We had the good fortune to erect and maintain for nine months

350 miles of field telegraph. The whole of that was brought in material from England; we could find nothing of service on the spot, not even a few miles of wire—of which we ran short—and practically the whole 350 miles of poles and telegraph line had to be sent from this country. I quite agree with what General Webber and Captain Beresford said as to the wonderfully good work done by the men. The telegraph, I am happy to think, surprised people with its rapidity in South Africa. We started at Barkly, which is the terminus of the Cape Colonial telegraphs, 700 miles from Cape Town, and went on at the rate of six miles a day* for 350 miles up the country. The total number of working men was only fifty-five, including those we had to leave behind at each of the fifteen offices as clerks and linemen. The first section that I took out, consisting of one officer only besides myself, and fifty-five men, with the assistance of a few natives, constructed 175 miles of field telegraph, and maintained it, with nine offices, before a second small section of another officer and twenty-two men could arrive from England. The total number of offices before the expedition returned was nineteen in the 350 miles. Sir Charles Warren always had his wire at the head of the expedition, and all movements of troops, stores, &c., were carried out by means of it; besides which the amount of private work done was very considerable.

In comparison with the Nile Expedition, we had the great advantage of working with one fixed administration; and here I wish to take this public opportunity of expressing my gratitude to Mr. Sivewright—a name well known to this assembly—who, after a most able career in that colony, in which he set the telegraphs on a footing similar to that in this country, has, I am sorry to say, resigned his position, to the great regret of all his friends. Mr. Sivewright helped me in every possible way, and by his aid we were able to work on a simple system by which all

* That is to say, an *average* of six miles a day for 58 working days, with two detachments only as a rule. One detachment of twelve men can count upon one mile an hour of air line telegraph even under unfavourable conditions, and at peace manœuvres as high a speed as three miles an hour has *been* obtained.—R. H. J.

messages taken by his department were sent and delivered over my line, his department receiving the money, and all messages taken on my lines were received by his system, our department receiving the money. No clearing house was required with this arrangement. The only slight complication was in regard to cablegrams, the cash for which we had to pay over to the department the same day as the telegrams were handed in.

I should also like to bear testimony to what General Webber has stated of the zeal and loyalty of the men who worked under us. I have the great satisfaction of knowing that while messages of the utmost importance were continuously passing along our lines of communication, so far as I have heard, from the beginning to the end of the expedition—a period of nine months—not a single message leaked out. As a proof of that I may state that the sad news of the fall of Khartoum reached me at Vryburg, 125 miles up our line, and was read off at several intermediate offices on a certain day. Sir Charles Warren, matters at that moment being exceedingly ticklish, was anxious that the news should not be generally known for twenty-four hours, and it is a great proof of the trustworthiness of the men that this order was implicitly obeyed.

The line we erected, although formed of temporary poles and wire, has stood in the most wonderful way. We "jumped" holes for all our poles, even the 15-feet poles; we had no digging; the butts were tapered and the holes jumped. The stations were about 20 miles apart, and the linemen stationed at them had standing instructions to gallop along the line on the occurrence of a fault of a quarter of an hour's duration, and it was a rare thing to have a fault existing for more than two or three hours at the outside. After the telegraph was handed over to the new administration, about fourteen men who knew something of the country volunteered to remain and take charge of the line, and it has continued to work as well as ever it did. A good income is now derived from it, and no doubt they will shortly be able to replace their wooden poles by iron poles of the Siemens pattern used in Cape Colony; and I have every reason to hope that that line will never be removed.

With reference to our takings. They were small and insignificant compared with those of the gigantic figures just read to us, but I am glad to say that in nine months we took £1,850 for private messages alone, which, considering that it was a desert, I think was pretty good.

It has given me great pleasure to hear the most able address of General Webber this evening, and, as a brother officer, I congratulate him most warmly on his success.

Professor G. Forbes: Nearly every one who has spoken this evening up to the present has been present at operations carried on by our army, either in Egypt or other parts of the world. Perhaps a few words may be listened to with patience from one who was not present in Egypt, but who has seen something of military telegraphs which may some day be in very close relationship with our own. The subject of General Webber's most interesting address has two departments: one is the exploring part of it, as it were, the establishing of the telegraph; and the other is the working of it when established. Concerning the latter part I have no experience, and nothing to say. Concerning the other part I have been very much interested indeed, and I dare say that the point which interested me has interested a good many of those who are here present, *i.e.*, what do we learn from all this as to the position of our military telegraphs compared with those of other countries with whom we may possibly come into collision? Consequently, while we were listening to this extremely interesting paper, I was comparing in my mind's eye what had been done in Egypt with what I had seen done in Russia, because, of all the difficult countries in which we are likely to have war in the future with any foreign country which is capable of using a telegraph, I think no country is more likely than Russia to be the one which we shall meet on a very formidable field so far as pioneering difficulties are concerned.

Now there were one or two things that struck me that the Russians were a little different in, but about this I could have no hesitation, and I think every one here will agree with me, that for indomitable pluck in overcoming difficulties where they can be *overcome* by the means at disposal, we may thoroughly back the

British officer and the British soldier. At the same time, it may be possible that another country less instilled with these instincts may have, by its nature, a greater experience in meeting difficulties, and therefore possibly greater forethought in preparing for emergencies. The first thing that led me to these comparisons was where General Webber spoke about leading a wire across the river at Assouan, a width of a mile. I should like to know, if General Webber would tell us, the nature of the wire which was used. I presume, from the remarks of Captain Beresford, that perhaps it was hard-drawn copper wire. One cannot remember the exact strain that there would be on a wire under those conditions, but I have so distinctly in my mind a wire, in Russia, laid under almost exactly similar circumstances, that I cannot help thinking that if the Russians had been going to some place in Afghanistan where there was a possibility of such a gorge to be traversed they would have had their wire ready for it. The place where I mean is at Krasnoyarsk, in Siberia, where a telegraph wire crosses the Yenisei, a distance a good deal over a mile, and with an island in the middle. To the best of my recollection the wire touched on the island, but of course the strain would be the same, except that the effect of the wind would be different. Another point that I was much struck with was the fact of there being no mention (or I missed it if there was) of light field telegraphs, which were spoken of as being used in Bechuanaland. The importance of this is very great indeed, and I know perfectly well that the field telegraph is used by our army; but if I understand rightly that light field telegraphs were not used in the Nile Expedition, it seems as surprising as that there were no telephones.

A statement was made by Captain Beresford that in the Soudan Expedition—I suppose that was a light field telegraph—for the first time a European army brought a telegraph wire into the field of action. Now I incline to think that this is not quite correct. I was present with General Loris Melikoff at the time of an attack on Mukhtar Pacha in 1877 (when Mukhtar Pacha's army was encamped, I mean, on the flanks of the mountain Aladja dagh, close to Kars), when he sent out my friend General

Lazareff—with whom I had a long intercourse on a separate expedition—to turn the position to prepare for attack on the morrow, and General Lazareff took a field telegraph with him on the march; and not only then, but on several occasions, I have seen the Russian army paying out this field telegraph, not at the rate of six miles a day, which has just been spoken of as done in Bechuanaland, but at two miles per hour, telegraph poles and everything required. I saw this in Armenia, and on the Black Sea coast in Abkhasia, so that I should like to have a little more information from General Webber as to the field telegraph and the rapidity of bringing the telegraph into action. By the bye, the bringing of that telegraph into action by General Lazareff was really supposed to have had very serious importance in the operations of the next day, which, as every one will remember, eventuated in the flight of Mukhtar Pacha and his army, and eventually in the taking of Kars. I hope that as a matter of fact we are as thoroughly ready as the Russians to enter an unknown and unexplored country, and to meet every possible difficulty which may occur in the way; but I know from personal experience, having been in nearly every part of the Russian empire, of the enormous struggle for existence in Russia, and the consequent power of getting over difficulties and of constant passive endurance, as well as anything else, which we, from want of practice, have not got used to to so great an extent. A Russian from the day he is born must be a hardy fellow; if he is able to stand his first year of baby life, from that year he is very tough, and can endure almost anything for the rest of his life; and so in a country like that, where a thousand miles is thought a mere trifle, where one drives a thousand miles as easily as we do a hundred, the difficulties such as those which are often mere difficulties of distance are comparatively small. I shall be very pleased if General Webber can assure us of our having made as careful preparations for unknown contingencies, for that is the real thing, in case of our being compelled to undertake a war in a thoroughly difficult and unknown country.

Capt. Beresford, R.E.: As regards the telegraph being *brought* into the fighting line, what I mean is the shooting line;

not only into the area of a battle, but actually into the line of fighting men. It was worked within three feet of men fighting, and Major Turner was as nearly as possible speared, and his horse was killed.

Professor FORBES: I am glad the honour is with our country.

Lieut.-Col. HAMILTON, R.E.: With regard to Professor Forbes's remarks as to the rapidity of putting up the field telegraphs, mentioned by Major Jelf as being only at the rate of six miles a day, I would remark that that was the rate when moving with troops on the line of march; but the actual manipulation of putting up a field telegraph line on poles can be done by the Royal Engineers Telegraph Battalion at the rate of three miles an hour under favourable circumstances.

The PRESIDENT invited General Webber to reply to the observations made upon his paper.

Major-General WEBBER, C.B.: It has been with great pleasure that I have heard Major Jelf and Captain Beresford address this meeting. I only regret one thing, and that is that they have perhaps told us to-night that which may prevent them on some other occasion considering it necessary to give us a more lengthy account of their operations in Bechuanaland and at Souakim. I can only deprecate any such prevention, and hope that they will put their pens to paper for the edification of our Society and our guests as soon as they can.

As regards Professor Forbes's remark about the span at Assouan. In my haste I probably touched upon that point too lightly, and did not explain the situation quite enough. That span was not put up by the Military Telegraphs, but by the Egyptian Administration. It was a No. 8 wire, and obviously a wire with a mile span and 330 feet sag could not do anything else but break. It was possible, of course, on such a span for a really hard-drawn piece of wire to be suspended and strengthened towards each support, but the sag would be something very different to what was attempted on that occasion.

As to Professor Forbes's reference to the work of the Russian army telegraphs under General Lazareff, when he was turning the flank of Mukhtar Pacha, an account of that operation was

given by me at the United Service Institution in 1878. I think it is one of the most interesting examples of the work of a field telegraph. If it was built at the rate of two miles an hour, it is proof that that rate was confined to the distance over which the army was marched. I forget now in what time the flank movement was made, but certainly it was a most brilliant operation, and no doubt the accuracy of the Russian general's simultaneous attack on the Turks was largely due to the existence of the field telegraph.

As regards working in difficult countries, I think that Professor Forbes may be reminded that the telegraphs of the British Army have worked in the past in the Crimea, in China, in all parts of India and its frontiers, in Abyssinia, in Ashantee, and in South Africa. Colonel Hamilton, who is present, remembers one of those campaigns, and knows what the work was like; and therefore I hope that Professor Forbes will allow that, like the Russians—who have a very varied experience o different countries—we also have experience, one which some of us may have the joy, if I may be allowed to say so, of utilising one of these days, when England shall fight Russia on her own ground.

The PRESIDENT: Gentlemen, I will now ask you to give General Webber a hearty vote of thanks for his most interesting paper. It is most gratifying to us as telegraphists to hear that our branch of the science is making itself so useful and doing such excellent service at seats of war; and from the remarks made by the various speakers it is quite evident that the greatest credit is due to General Webber and those gentlemen who have had charge of this expedition, as well as to those who assisted them. I will ask you to let your vote of thanks include also those gentlemen who have taken part in the discussion.

The proposition was heartily carried.

The meeting then adjourned until Thursday, November 26th, 1885, when a paper was announced to be read by Professor J. A. Fleming on "The Necessity for a National Standardising Laboratory for Electrical Instruments."

At a ballot which took place after the Extraordinary General Meeting held on June 11th, 1885, the following were elected :—

Foreign Members:

Saitāro Oï. | H. Renisch.

Associates:

E. J. Silcock. | Ebenezer Stiff.

Student:

H. T. Rutter.

The One Hundred and Forty-ninth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 26th November, 1885—C. E. SPAGNOLETTI, Esq., M.I.C.E., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

Donations to the Library were announced as having been received from the Smithsonian Institute and Dr. D. Tommasi, to whom the hearty thanks of the meeting were duly accorded.

The following paper was then read:—

ON THE NECESSITY FOR A NATIONAL STANDARDISING LABORATORY FOR ELECTRICAL INSTRUMENTS.

By J. A. FLEMING, M.A., D.Sc., Member.

"If every scientific inquirer observed only for his own satisfaction, and reasoned only on his own observations, it would be of little importance what standards he used, or what contrivances (if only just ones) he employed for this purpose; but if it be intended (as it is most important they should) that observations once made should remain as records to all mankind and to all posterity, it is evidently of the highest consequence that all inquirers should agree on the use of common standards, and that these should not be liable to change by lapse of time.

"Unless we transmit to posterity the units of our measurements, such as we ourselves have used them, we in fact only half bequeath to them our observations. This is a point too much lost sight of, and it were much to be wished that some direct provision for so important an object were made."—SIR JOHN HERSCHEL.

In the above quotation from his famous "Discourse on the Study of Natural Philosophy," Sir John Herschel gives emphatic and elegant expression to the general idea which it is proposed in this paper to consider, with special reference to the quantitative study of the sciences of electricity and magnetism, and especially with reference to the world-wide practical and industrial applications of them, in which the employment of accurate measurement forms the basis of all success.

The early pioneers in the region of electric measurement arbitrarily selected their own standards and units of comparison. Cavendish possessed, as we now know, a complete set of electrostatic condensers, or standards of capacity, whose values he expressed in "globular inches," or in terms of the capacity of a sphere one inch in diameter. He also expressed what he called "degree of electrification," or, in modern language, "electrostatic potential difference," in terms of certain deflections of idiostatic electroscopes of the kind known as Henley's.*

Wheatstone expressed the results of his investigations on electric conductivity in terms of a certain copper wire, one foot in length, and weighing 100 grains.†

Jacobi employed a similar standard, called "Jacobi's Étalon," which was a certain copper wire 22·4932 grammes weight, 7·61975 mètres long, and ·667 millimètres diameter; copies of which were made and deposited in various places.‡

And before the introduction of the units of the British Association system, based on the absolute system of Weber and Gauss, units such as the Daniell's cell and a mile of No. 16 B.W.G. copper wire were common standards for practical use. We have now passed out of the period when the quantitative study of electricity is confined to the laboratory and the technical school. The physical quantities there dealt in are now actualities affecting public convenience and life as much as the yard measure or the imperial pint. The physical realities of which these measures are the expression, however, are no longer the subject simply of philosophic discussion, but enter into important operations affecting seriously public welfare. An amount of electric energy is now as marketable a commodity as a loaf of bread, and is as much the subject of purchase and sale as a barrel of beer or 1,000 cubic feet of gas. This being the case, a very important consideration rises into view.

---

* "The Electrical Researches of Henry Cavendish," edited by Clerk Maxwell, introduction, p. xxx; p. 279, *et seq.*

† "Wheatstone's Scientific Papers," reprinted by the Physical Society of London, p. 110.

‡ Maxwell's treatise on "Electricity and Magnetism," vol. i., p. 427, second edition.

If an explorer is starting on an expedition or voyage, he does not content himself merely with the purchase of a chronometer or a sextant from an eminent maker as affording him perfect certainty of observation. He sends the instrument to Greenwich or to Kew, and obtains a careful comparison with the best authoritative standards; and the Kew certificate of a thermometer is an essential accompaniment of every instrument to be applied to accurate research. We do not in these cases feel satisfied to rely on the fact that the instrument has proceeded from the workshop of some well-known manufacturer. We can obtain, by the payment of a fee, an independent statement and table of errors made by comparisons with the very best standards of comparison which human skill and knowledge can possibly construct. But with the exception of a beginning which has been made in this direction at the Cavendish Laboratory, Cambridge, there are no corresponding facilities whatever for placing a hall-mark of efficiency upon an electrical instrument intended for measurement. Every month, hundreds of instruments are being made for the measurement of current, electro-motive force, capacity, resistance, and power, of which we have no other guarantee of their reliability than the confidence we may happen to repose in the reputation of their maker.

Those who have time and facilities at their command, and, above all, the necessary knowledge, do, no doubt, abundantly test the instruments they work with, and satisfy themselves as to the correctness they require; and the knowledge of what instruments may be depended upon does, no doubt, to some extent filter downwards and become a sort of oral tradition. But the fact remains that the process of multiplying standards and issuing measuring instruments is subject to no other control than the pains which may happen to be taken by those who actually, with their own hands, make and test these instruments.

It is very difficult for any one to learn what is the exact degree of accuracy or pains which has been taken in the first realisation of the standards and instruments possessed by each maker, and of which those issued to the public are approximate copies. There is no one to insist that any one who takes upon himself to issue *to the* world electric current meters shall have taken any more

pains than he likes in the construction of the standard, or the selection of the process which furnishes him with the estimate of what is one ampère of electric current. It is not open to any one to have independent views on what constitutes 18-carat gold or proof spirit. The proper authorities in this case step in and certify to the correctness or incorrectness of these views. But, judging by the results of experience on multitudinous instruments, there are a very large number of different views as to amount of current or electric pressure which it is proper to call an ampère or a volt, and in this case there is no court of appeal, and no goldsmiths' hall nor excise office, to be the umpire in case of dispute.

The wide diffusion of Greenwich time makes it comparatively easy to recover in any place in the kingdom the mean solar second. The not very difficult task of testing a good balance, and the process of double weighing, enable comparisons of mass to be made with weights which can be checked against Board of Trade standards; whilst the extensive use of the standard gauges of Sir Joseph Whitworth makes the measurement of dimension in a common unit a matter of very general custom. Although, however, the identity of unit in the case of the fundamental measurables is very fairly exact, there is a large gap yet to be filled before the practical measurement of electricity obtains anything like the same universal exactness as mass, length, and time. We must not in this matter allow ourselves to be led away by phrases and words, and assume things to be equal merely because they are called by the same name. It cannot be too carefully borne in mind that the value of Gauss's so-called absolute system is in enabling theoretical calculations to be made without the use of a multiplicity of constants; and the value of the British Association system, which aims at making the practical units as nearly as possible decimals of these, is in enabling rough calculations to be made without other constant factors than unity and powers of 10. But since the actual standards are not equal to the theoretical standards, we come back to this position: that all standards now in use are just as arbitrary as Cavendish's or Wheatstone's, and as a practical thing it is even of more importance

that we should all work from the same physical standards than that the divergence of these from the theoretical units should be accurately known. Most people are beginning, for instance, to see that no great practical result was effected by the Paris Congress in changing the unit of resistance from the mean B.A. standard to the new legal ohm. The classical labours of Lord Rayleigh and those associated with him had already determined the proper relation of the mean of the fourteen British Association standards of resistance to the centimètre and the mean solar second, and that alone was sufficient for the reduction of observations to absolute measure; and the results have been substantially confirmed by Rowland in America. The introduction of a whole fresh set of copies of a unit differing about 1 per cent. from the mean B.A. unit has resembled the effect of introducing a fresh yard measure about $\frac{1}{3}$rd of an inch larger than the old yard, so that the question would always have to be asked, What yard do you mean?

The absolute value of the practical units is of no more consequence than the relation of the mètre to the earth's meridianal quadrant. The actual standard of length, as defined by the French Government on the 2nd November, 1801, is the distance between two points on a platinum bar at 0° centigrade, and *not* $10^{-7} \times$ an earth quadrant.

The actual standard of mass is the standard platinum kilogramme des archives made by Borda, and *not* the mass of the cubic decimètre of pure water at 4° centigrade.*

In his report to the Royal Society on the work of the British Association Committee of 1861 on Electrical Standards, the late Professor Fleeming Jenkin urged these considerations with great force and clearness. He says, referring to Jacobi: † "He

* The unit of mass in this country is defined by the Act of Parliament 18 and 19 Vict., c. 72, July 30th, 1855, to be a piece of platinum marked "P. S., 1844, 1 lb.," deposited in the office of the Exchequer, which "shall be, and be denominated, the imperial standard pound avoirdupois." One seven-thousandth part of this pound is the grain. The French standard of mass is the kilogramme des archives made by Borda. Professor Miller found the kilogramme equal to 15432·34874 grains. These principal standards are now deposited in the Standards Office of the Board of Trade.

† *Comptes Rendus*, 1881, vol. xxxiii., p. 277. Also see "Reports on Electrical Standards," edited by Fleeming Jenkin, London, 1873.

pointed out with great justice that the mere definition of the standard used as a given length and weight of wire was insufficient, and that good copies of a standard, even chosen at random, would be preferable to the reproduction in one laboratory of a standard prepared and kept in another."

Now what is true of a standard reproduced is also true to a certain extent of uncertified copies of standards, and copies of standards whose pedigree is, so to speak, unknown. We are depending on the exactness of comparisons made by unknown persons in various workshops for the multiplication of working test instruments and units, and not, as should be, upon certified values attached to these copies after comparison by authorised observers with one single standard—observers who have no other interest than that of making this comparison as accurately as possible.

The work of the British Association Committee abolished the multiplicity of arbitrary units, and substituted a common agreement as to the magnitudes to be represented; but electric science has advanced by such leaps and bounds that what was twenty years ago a marvellous unity is now known to admit of far greater approximation towards perfection. And this discrepancy between instruments and standards is by no means a small matter. Consider it for one moment from the standpoint of pure science. We rely for the ultimate testing of our theories of natural operations upon the comparison of the results of theory with those of experiment. The most convincing of those agreements arise out of concordant experimental results arrived at by different methods. Unless there is absolute certainty upon the physical identity, and not merely nominal identity, of the units used, it is most difficult to place alongside the experimental results of different observers in different countries with perfect confidence that they are dealing with identical magnitudes. Agreements or differences may thus be covered up which, if revealed, would be suggestive for fresh, and perhaps valuable, work. Consider it again from the point of view of applied science. It is remarkable that the industrial applications of electrical science have forced into existence a standard of accuracy far greater than would ever be attained in mere

laboratory work. It is a mistake to suppose that measurements in practical and commercial work are less exact than those undertaken for purely scientific purposes. It was the attempt to make submarine telegraphy a commercial success which forced and demanded an accuracy in the measurement of electrical resistance and capacity never before desired; and it is electric lighting which has developed a corresponding necessity for accuracy in the measurement of current strength and electro-motive force. And applied science has paid back with interest the demands thus made on the abstract and pure science for assistance. Owing to the exactness of the measurements thus demanded we know, for instance, more about the flow of electricity in conductors from the everyday phenomena of submarine cables than could have been obtained in half a century of study of laboratory effects. And it behoves those who have the interests of pure science at heart to see that tribute is exacted from the countless operations of industrial science, by making its measurements, undertaken in practical operations, readily available for the uses of theoretical science. And in no way can this be done except by the earnest effort to obtain and enforce the highest possible accuracy and unity in commercial measurements, based upon and referred to known and common standards.

Assuming now that enough has been said, even if anything need be urged, to show that some provision should be made to meet the need, the next question arises, How is it to be done? And the solution of the problem I am about to ask you to consider is, that it will best be obtained by the establishment of a national standardising laboratory for electrical test instruments. Let me give you, in the first place, a sketch of what seems to me should be the constitution and work of such an institution; next consider what should be its relation to existing corporations or institutions; and, lastly, how it should be set on foot.

Passing for the moment over the question of ways and means, let us assume that somewhere or other in the neighbourhood of London suitable premises are acquired, fitted with the usual arrangements of a physical laboratory.

It should be on ground not liable to vibration, convenient of

access, and in telegraphic communication with the observatories of Greenwich and Kew. Let us assume that such a place is provided with such fundamental standard instruments as a clock regulated from Greenwich, with balances of the best description, and with carefully-tested standards of mass; with such instruments as a cathetometer, standard gauges, standard measures of length, and the most approved forms of spherometers, callipers, and micrometers, for the fundamental measurements of mass, length, and time. We will assume, further, provision is made by a dynamo at some distant place for supplying secondary batteries as a source of current and electro-motive force. The next step would be to design and construct instruments for the various standardising operations.

In the work of designing these one especial consideration would be to obtain the advice, and perhaps assistance, of those scientific men who in each case are known to have made a special study of that class of measurement.

Nothing ought to be done without a general consensus of opinion on the part of those best fitted to judge of the form such standard instrument should take.

It would be necessary to obtain the assistance both of experimentalists and mathematicians in those cases where calculations are necessary to convert the observed instrumental indications into absolute measure. If we assume that it is possible to do this, and that such work is done once for all in designing, computing for, and setting up such instruments as an absolute gravity electrometer, or a gravity voltmeter on Sir W. Thomson's plan, or a current weighing balance and coils on Lord Rayleigh's method, then ever afterwards the measurement of a difference of potential or a current in absolute measure is reduced to simply weighing a certain attraction, and assuming gravity to be constant, a permanent standard of reference is obtained.

If galvanometric methods are adopted, or galvanometers have to be tested, provision would have to be made for the constant record of terrestrial magnetic force.

In any case, however, the routine operations could be

performed by careful observers, who need not, however, be supposed capable of conducting either the calculations in the first place or the construction of the standards.

The plan consists, therefore, in having permanently available the apparatus employed by those experimentalists whose methods have commanded the greatest confidence from all who are fitted to appraise them. It is hardly necessary here to enter into a full discussion of what methods would best be employed in the construction of permanent apparatus of the best and most solid character for the comparison with the most carefully constructed standards of resistance, capacity, electro-motive force, self-induction, electric power, and current strength and quantity. The British Association Committee on Electrical Standards includes the names of most who have directed attention to electrical measurement, and these, with the addition perhaps of other names, would form an advising board whose opinion would be sought and taken on the best methods to be adopted in setting up such permanent standards. Now, such opinion, embodied in facts, could not fail to command general confidence; and if this can be created, and the necessity for following its advice felt, it might come in a short time to be generally appreciated that instruments and standards would have additional value by having been certified as compared with these permanent standards, and a moderate fee sufficient to cover expenses of such certificate would probably be no more grudged in the case of current or ampère meters than it is in the case of thermometers. Up to the present time this has only been attempted with resistance coils. The British Association Committee on Electrical Standards have arranged that units of resistance can be compared at Cambridge with the units there preserved. What we are desiring is an extension of this to other electrical instruments, and no provision has as yet been made for that. What has first got to be done is to wake up the attention, not of highly trained and competent electricians such as the members of this Society, but the electric public generally, who are in every direction beginning to use and demand instruments for the necessary measurements.

The necessity for this general attention to the question is far

more urgent now than it was five years ago. Ten years ago hardly anyone had ever seen a direct-reading instrument, and such commercial measurement as was done in telegraphic work was in the hands of competent electricians, who were quite alive to the advantages and necessity of unity in measurement, and who strove to attain it to the best of their ability. But the case is widely different now. If it is possible to arouse a general public sense concerning the advantages of such independent standardising, it might not be an impossible task to set on foot the proper organisation, and no body is so competent or so well able to create this opinion as the Society of Telegraph-Engineers and Electricians.

In doing this we shall only be supplementing and extending the work of the British Association Committee on Electrical Standards. This committee, which has now sat continuously for 24 years, has accomplished work which is an enduring monument to British science, and if, in extending it by the foundation of a national standardising laboratory, the same unity is effected in commercial and general work which the committee effected in scientific measurement, it would certainly be found that other countries would follow suit, and that England had again taken the lead in a matter of extreme importance. One objection may be raised, namely, that it would be impossible to standardise instruments which by the very principle of their construction were liable to change, and that a certificate of efficiency might be, in this case, even misleading. The best way to meet the difficulty would be to complete the standardising in stages, allowing the owner to have back the instrument in between and use it. If then the scale errors were found to be different each time, it would call the attention of the user to the non-permanence of the value of the scale readings. Also some note of caution might be marked on the certificate as to the circumstances under which the instrument was liable to be in error.

My object is not, however, to enter into details, but to throw down general principles for your consideration.

Next let me draw attention to two other uses to which such a laboratory could be put.

First, the testing of electric materials. If anyone wishes to buy iron ore or caustic soda, there are numerous laboratories to which he can send a sample and get the analysis certificate by the payment of a moderate fee. There is no corresponding institution to which a sample of insulated copper wire, a sample of iron, or a sample of gutta-percha, can be sent to obtain a certificate of its electrical qualities. Large firms and companies no doubt do this for themselves, but there is an immense quantity of private work being done which I venture to assert is not based on any sound knowledge of the quality or durability of the electrical materials used. The apparatus for making these tests is costly, and can only be used by specially trained observers; but as electrical work falls more and more into the condition of a general trade, it will become most essential that some provision should be made for doing this, and for doing it by some institution which, like Cæsar's wife, should be above suspicion as to bias or carelessness.

Next, there is a second field of most important national work which might be carried out, and that is the special and systematic undertaking of the determination of electrical currents.

At present this class of work proceeds but slowly, and by disjointed stages. There are but few scientific men who can get the necessary continuous time to prosecute researches of this class.

Those engaged in teaching or industrial work find their time too much filled up with professional duties. Sometimes they are able to obtain assistance from advanced students and assistants, but most generally such help is not continuous enough, nor always available. Many others who have the leisure have not the necessary appliances and place. Quantitative work of the kind referred to is useless unless undertaken with the best possible apparatus and under the conditions conducive to continuous work. Very occasionally it happens that all the conditions and qualifications unite in one place and one person, and then we obtain such perfectly priceless work as that by which Lord Rayleigh has earned the gratitude of all who are connected with electrical science. But if we assume that a laboratory

existed in which there was no teaching and no general research to interfere with or disturb certain competent observers, and if we assume the place provided with the most complete appliances as above suggested, it would be possible for those who know what is wanted to set certain pieces of definite work to be accomplished, and they might then find time and thought to direct the continuous operations of observers who would work under them.

Take, for instance, the question of specific inductive capacity. What a whole range of problems still await solution! Not only are the dielectric constants wanting for many insulating media, but the effect of temperature, pressure, time of electrification, varying electro-motive force, and other variations, have yet to be determined in a systematic manner for all.

These data would form immediate guides to right action in practical work. Supremely important would such data be in testing the magnificent electro-magnetic theory of light of Clerk Maxwell. Although it is now just twenty years since that theory was first published in the *Philosophical Transactions*, we seem as far as ever from established proof of some of its fundamental propositions. One of the fundamental assumptions is that in dielectric media what is called the electric displacement is equal to the electro-motive force multiplied by $\frac{K}{4\pi}$, where K is the specific inductive capacity; and a corollary of its fundamental assumptions is that in transparent media the dielectric capacity should be equal to the square of the index of refraction.

The divergence between observation and theory in this last result indicates that much more extensive observations are needed to enable a comparison to be made, with the hope of finding a clue to the reason for this divergence, or for the correction of the theory.

Considering what vast consequences might not result from an arrival at some true conception of the nature of the motions which constitute an electric current, is not the steady prosecution of those quantitative observations which can test our theories a thing to be definitely undertaken, rather than suffer them to take their chance amongst other voluntary work?

In spite of the valuable quantitative work of Stoletow, Rowland, Ewing, and others, how much yet remains to be cleared up on the subject of magnetic permeability and susceptibility of different kinds of iron and steel! The choice of an iron for a field magnet of a dynamo, or for a telegraphic wire, should be based on accurate knowledge of the value for that sample of these quantities. The highly interesting researches of Mr. Preece on the velocity of signalling through iron and copper wires have revealed in a striking manner that which was known to theorists, and which has been fully explained both by Maxwell and Kirchoff, namely, the effect of magnetic permeability on the self-induction of a conducting circuit. The quality of iron required for a telegraphic wire is therefore completely different to that required in a dynamo machine—in one case the smallest possible magnetic permeability consistent with other requirements, and in other cases the largest.

The attention that has recently been drawn by Mr. Bottomley, and previously also by Dr. Hopkinson, to the effect of the presence of manganese in iron or steel in affecting its magnetic qualities, is of the highest importance in regard to these points.

On all these questions further complete quantitative observations are supremely important, and would have an immediate bearing on national industries. The reason for their absence is obvious. The difficult character of the quantitative researches needed prevents them from attracting most investigators, and such experiments would be too tedious and not sufficiently immediately remunerative enough to invite private firms or corporations to undertake them. When we look back and review the many splendid investigations which distinguished French science at the end of the last and beginning of this century, we see the results of the definite action of the Academy of Sciences and the State in setting on foot such kinds of quantitative research. It is not impossible that such a scheme might be enlarged so as to include definite researches undertaken to order; but in considering this question for the first time, it is perhaps well to keep our ideas as compact and definite as possible, and I have accordingly said *nothing* about such an important work as testing dynamos and lamps.

There have been schemes put forward before for public laboratories, but they have always fallen through on account of the fact that every one feels that any broad ill-defined scheme of endowment of research is an unpractical thing, and that there is too much danger that endowment of research in this fashion would simply degenerate into a research after an endowment. If anything more than conversation about the subject is to take place, it is obvious that no scheme can be complete which does not face the difficulty of ways and means, and unless we can compass this natural difficulty nothing can be done. It will be an important question to consider, in the first place, whether such a scheme should be affiliated to any existing institution, or whether it had not better be something quite apart and distinct.

With respect to the first capital outlay, our only choice lies between private munificence and State assistance. Considering the considerable revenue that the State has derived of late years from patent fees for many thousands of electrical patents, is it too much to ask that a little of that which has been gained by the industry of inventors should come back in the form of an endowment of an institution which will be largely useful in providing and obtaining data upon which research and invention can be based?*

The few thousand pounds necessary would hardly be appreciably felt by the British taxpayer, whilst the advantage to the community would be cheaply purchased.

---

* The total numbers of British patents taken out in the last four years are as follow:—

In 1881	...	5,751	patents.
„ 1882	...	6,241	„
„ 1883	...	5,993	„
„ 1884	...	17,110	„
Total number	...	35,095	

I learn from a report by Mr. Frost, our Librarian, that the numbers of the electrical patents during these years were as follow:—

1881	...	450	making a total of 3,010 in all.
1882	...	845	
1883	...	620	
1884	...	1,095	

Considering that we have now arrived at a condition when applications of electricity enter into countless departments of life, the question to be considered is whether, if private enterprise fails to accomplish it, it is not the duty of the State to add to its control over other weights and measures that of some supervision over electrical measurement. It will certainly have to do so if ale of electric energy by meter ever becomes an extensive thing, as it will be absolutely necessary to have a court of appeal if dispute arises between supplier and customer; and if that is to be done, the matter cannot be limited to quantity meters alone.*

The foregoing remarks have probably sufficed to outline in a very rough way the present difficulty and some proposed remedy. I conclude by casting into the form of five propositions the notions above thrown out, not in any way intending them to be dogmatic statements of what ought to be accomplished, or the way to do it, but simply as fulcra round which your discussion can turn, and they are as follow:—

1. The measurement of electrical phenomena and effects has now become such a practical and everyday operation that it is highly desirable that unity of operations should be secured in practice as well as theory.

2. That in order that this should be the case, certain actual acknowledged standards of reference or apparatus should exist, which should be to electricity what the Greenwich mean solar standard clock is to all timekeepers, or what the standard yard or pound is to length and mass measurement.

3. That this can perhaps best be attained by the establish-

* Considering the fact that central station lighting, both arc and incandescent, has now been carried on for three years in New York, it is not too much to assume that under a revised Electric Lighting Act, which must be obtained, attempts will be made gradually in England to supply the desire for distributed lighting. Whether the sale be by current, by energy, or by light, it is certain that no solid basis for commercial contract can exist unless the quality and quantity of the thing sold is defined beyond dispute, and that, in case of doubt, standards of reference exist which can be referred to. The question is surrounded by much difficulty, and such rough notions as we have already on the testing of incandescent lamps will have to be refined and improved before the matter is in a satisfactory state.

ment of a national electrical standardising laboratory as above described.

4. That action in this matter should be taken by the Society of Telegraph-Engineers and Electricians as representing practical science, in conjunction with the British Association Committee on Electrical Standards.

5. That the Council be requested to give the matter their consideration, with the view of seeing if there is any practicable method of meeting these requirements, and, if need be, to confer with the members of the British Association Committee on Electrical Standards and the Council of the Royal Society, or other acknowledged authorities, to see if it commends itself to these bodies as a scheme of practical utility or of national advantage to have undertaken.

The PRESIDENT: I will now ask you to be good enough to discuss the paper which Dr. Fleming has been kind enough to bring before us. For my own part, I think he has made out a very good case, and has shown the very great necessity of, and the advantages that will arise from, a recognised laboratory of the kind which he has shadowed forth this evening. It will add to the value of measurements if they are accurately made by instruments which have been proved and found correct; and the convenience of having such an institution, where at any time apparatus can be compared, would be very great and useful.

If each class of instruments were made to a standard, it would be a very easy matter to obtain or give confirmatory results; but at present many of the instruments in ordinary use vary so much that little confidence in the statements of tests is felt until it is known by what instruments the results were arrived at, and the condition of those instruments.

There are several gentlemen present to-night who will be able to give us some very good advice on the matter; and we are favoured with the presence of Mr. Chaney, who is the head of the Standard Department of the Board of Trade. He will doubtless be able to give us some information as to the value of standards which will tend to help our subject very much. I

think that the suggestion which Dr. Fleming has made, of having a very wide committee to consider this question, comprised not only of members of our own Society, but of those of the British Association, the Royal Society, and others, may be a very excellent mode of getting together such influence and advice as an important subject of this kind ought to have.

Mr. H. J. Chaney: I think it is evident that there is practical necessity for uniformity in the measures and apparatus used for electrical purposes, although I am not quite sure that the paper which I have had the pleasure of hearing to-night shows with sufficient force the number of complaints or representations that have been made in that direction. It is clear that we do not require the determination of new units, or the definition of new standards. These questions appear to have been so far settled by the devoted labours of the British Association Committee on Electrical Standards, and perhaps to some extent by the propositions of the International Conference on Electrical Units. What we do require is practical uniformity in the measures that may be used for the purposes of trade, taking trade in its wider sense connected with the development of electric lighting. It is not for me to say how far the Board of Trade would be prepared to meet the views that Dr. Fleming has put forward. It is their duty under the present Act to take care of all matters relating to weights and measures; but the question he has proposed is a somewhat elaborate one, and I think it would be desirable to separate the proposal with reference to obtaining electrical standards, maintaining them, and using them, from the proposal to have a national laboratory where researches and highly scientific inquiries might be carried on. The latter proposal tends, I think, rather to embarrass the other. If Dr. Fleming simply proposed to have an office of some kind, whether in London or elsewhere, where these electrical measures and apparatus may be tested, I, for one, do not see so much difficulty in that: the expense is not perhaps a considerable one, the instrumental equipment is not very difficult, and the question as to what extent fees should be charged or not for the verification of standards is one that might be fairly considered. At present no fees

are charged at the Standards Office on anything done there for recognised authorities or for scientific purposes, and I do not see why fees should be charged in the important verification of electrical standards.

Perhaps the case referred to by Dr. Fleming, of the testing of watches, is hardly analogous to that of testing electrical meters, for, in the case of a watch, the instrument tested is not used in trade transactions or in the exchange of value: persons, for their own convenience, have watches tested, and the testing meets their satisfaction; but in a measure which is used for the purpose of trade we must have a test which will not only be of use to the seller—that is, one person—but also to another person, the buyer; and that test can evidently only be recognised as authoritative when it has legal force. I am not at present aware that there is any Act of Parliament which would directly give legal force to the testing of electrical measures or apparatus. It might perhaps be possible, if standards were constructed and approved under the directions of a joint committee of this Society and of the Royal Society, and deposited in due form, that they might then be legalised by an Order in Council, in the same way as has been done with the Whitworth standards and others; but as to how far the Board of Trade would be prepared to go in that direction I am not at present able to say. It is a matter, however, to which they would be prepared, of course, to give careful consideration to any recommendations which might be made to them.

The PRESIDENT: Perhaps Mr. Whipple, of the Kew Observatory, will favour us with his remarks.

Mr. G. M. WHIPPLE: Sir, seeing before me at the Council table two members of the Kew Committee of the Royal Society, the body which controls the operations of the Kew Observatory, I think it is perhaps scarcely my place to stand up here; but having been called upon by you, I will venture to make a few remarks.

It appears to me, after having heard Dr. Fleming's paper, that the Kew Observatory already stands in a position that would enable it very easily to become a suitable institution for comparing electrical standards. I quite agree with Mr. Chaney that

it is advisable that the question of a national laboratory for investigations should be detached from the question of comparing instruments, and will not speak upon it. The Kew Observatory complies with several of the conditions laid down by Dr. Fleming as requisite in a testing office. It has standards of time; we keep our own time, obtained directly by astronomical methods, in order that we may be enabled to compare watches for the public. We have our own standards of length and weight, which are copies of the Government standards, compared with official standards (in fact, are numbered as official standards) by the late Mr. Sheepshanks. We have magnetic instruments which are constantly recording the intensity of the earth's magnetism; and we have, of course, standard thermometers, and various others of the standardising instruments that would be required to complete the outfit. The observatory is also in an isolated spot, being at a distance of nearly three-quarters of a mile from the nearest railway, street, or road of any kind; there is nothing present or in the neighbourhood which could cause tremors, vibrations, or other disturbances which would be likely to affect the accuracy of measurements, as is the case of those made in a laboratory situated in a town, or even situated in such a spot (Westminster) as the Standards Office which Mr. Chaney controls.

Then, as regards its communication with London, we have a service of something like 300 trains, by six different routes, daily between Richmond and London, and the actual travelling time that the Kew Observatory is distant from this building is just under an hour. Therefore there are several conditions which I think show that there would be a considerable possibility of establishing standards of electrical comparison at Kew. Our operations, as I have already said, are controlled by a committee of the Royal Society, and that committee have on many occasions shown themselves very anxious to take up any new branch of work that has been called for. I need only quote from a public statement I have before me to show the variety of instruments that they have been called upon to test, in extension of the original scheme of testing, which was, I believe, first founded upon that *statement* of Herschel's with which Dr. Fleming commenced his

paper. At all events, in an abstract given in the history of the observatory, which has been recently published by the Royal Society, I find that there are no less than twenty-six different classes of instruments we are now examining, verifying, and certifying, without including watches, because the comparison and certifying of watches is quite new work, and has only been recently started. The testing of watches, which has been taken up with great eagerness by the trade, was originally set on foot by the Horological Institute, which I presume holds a somewhat kindred position with regard to watches as this Society holds with regard to electrical instruments. Speaking of watches, I would just make a remark called for by Mr. Chaney's statement that there was no commercial value attached to a certificate given for watches. I think, if he were to inquire of a watchmaker for a Kew certified watch, he would find that the Kew certificate had placed a considerably augmented value on the watch; at all events, I know such has been reported by persons who have made the inquiry. Watch manufacturers have also stated that they are unable to obtain such a price for the watch as they would like to, in many cases, unless it has a Kew certificate.

With regard to the funds—a question which Dr. Fleming rather overlooked—I suppose that if a very strong representation were made to the Royal Society, there might be a possibility of obtaining a grant from the money they have at their disposal. In years gone by, when the original standards at Kew were procured, the Royal Society contributed very freely to their purchase, and I should presume that in this case the Royal Society could act in a similar manner, and might be induced to give grants for the initial outlay. Then, as for maintenance, I presume that purchasers of electrical apparatus would no more grudge the small additional fee for standardising than do purchasers of watches, sextants, thermometers, hydrometers, &c., grudge the additional few shillings that are now required for the purchase of certified instruments. I think it may be admitted that in every case the maker of a certified instrument gives considerably more attention to the construction of the instrument when he knows it is going to be certified than

when it is not, and that the purchaser of a thermometer gets more than the value of the certifying fee when he buys a verified instrument.

The President: Dr. Fleming gave us some very startling figures as to the increase in the numbers of patents taken out under the new Act, and as Dr. Jackson, of the Patent Office, is here, perhaps he will give us some corroboration and details of the information which Dr. Fleming has been good enough to lay before us.

Dr. M. J. Jackson: I believe, Sir, that Dr. Fleming's estimate of the number of electrical patents is substantially correct—perhaps rather an over-estimate than an under-estimate. By taking the numbers in the indexes one is liable to get the same patent repeated, and of course those indexes include some things which we would not usually discuss here, such as magnetic hair brushes, and electrical appliances to be worn on the person. I think, also, speaking roughly, that the amount which has been earned by the State in the matter of electrical patents has been perhaps over-estimated, as so many have not been completed or proceeded with beyond a comparatively early stage. Dr. Fleming's suggestion that the earnings of the State in one department should be used to promote its welfare in another department, would seem to be a good one in theory; but it is, of course, for societies such as this to promote that object.

As to the remarks about the legal and the British Association ohm, I think that the discrepancy between the B.A. and the theoretical ohm (which is to be deplored) was due to the go-ahead spirit of the committee of the British Association, and of course this go-ahead spirit has its disadvantages; and the question arises whether the time has arrived for the establishment of such a laboratory as that suggested by Dr. Fleming. I think we may certainly say that it has arrived. The British Association committee issued a standard which was perhaps a little premature, but we could now issue a very accurate one. Although I say it with some diffidence here, I would suggest that it is rather a useful and a fortunate thing that the development of heavy electrical engineering was in its infancy at the time of the British Association com-

mittee, as there would have been more difficulty in establishing a really scientific system of units if the practical demands of heavy electrical engineering had to be met than was actually the case. I have heard engineers accustomed to talk of horse-power rail somewhat at the use of so small a unit as the erg; but the use of a suitable multiple of the fundamental C.G.S. unit ought to meet their views in all cases.

With regard to the suggestion that Kew should take up the duty, I think it is to Kew that we would naturally look in such a case as this; but we have heard something this evening about dynamos and secondary batteries, and, having regard to the magnetic observations that are made at Kew, I would ask Mr. Whipple if there is plenty of room to keep the two buildings in which the different classes of measurement would be made apart at a sufficient distance.

Mr. A. J. Frost (Librarian): May I mention, Sir, that it is a part of the daily work in the Library of this Society to compile lists of electrical patents, and my reports for the last four or five years have contained statistics of the number of patents which refer to electricity. In addition to these compiled lists, we endeavour to put upon the Library table all the electrical specifications which are published each week; and the specifications in the Library comprise everything, as far as we are able to judge, relating to electricity, including even electrical hair pins, electric dolls, and such patents referred to by Dr. Fleming. It is difficult to discriminate as to the value of electric patents, and it is not for us to do so. What we endeavour to do is to make our collection of electrical patents as complete as possible, and to keep them up to date, for the use of the members of the Society and others.

Professor G. Carey Foster: After the way in which Dr. Fleming has brought this subject before us, there seems to me to be very little left for anyone else to say; but I am glad to have the opportunity of saying how admirably I think the matter was brought before us, and to thank Dr. Fleming for his paper. The question is, of course, one of which the importance has been recognised for a considerable time, and of which the urgency, we

may say, is now becoming very great. It has been mooted in this Society more than once in a more or less definite manner. I myself had the honour to give an address to the Society a few years ago, and I referred to the matter on that occasion. It is a matter which I think the Society of Telegraph-Engineers might very well take up. Dr. Fleming, with minute acquaintance with the requirements of the practical side of the question, has treated it to-night very fully and effectively; and I hope that his recommendation that the matter may be thoroughly discussed in a serious practical spirit by the Council of this Society will not be lost sight of, and that we shall have some practical step taken as the result of this meeting.

Captain P. Cardew, R.E.: I have not formulated anything definite on the subject, but I am fully in accord, if my concurrence is of value to anybody, with Dr. Fleming. I should be very pleased indeed to be able to obtain instruments with some kind of certificate that they would continue accurate. It is not so difficult to get an instrument which is accurate when you obtain it, but it is very difficult to ensure that it will remain so; at any rate, that has been the case with a great many instruments I have had.

Mr. R. E. Crompton: I heartily agree with every argument that Dr. Fleming has brought forward in favour of a national establishment for standardising electrical measuring instruments; and I go further, and say that the necessity for such a standardising establishment is so great that, if the Government does not move in the matter, we, as English manufacturers, must of necessity combine and do the work ourselves. We cannot go on any longer as we are now doing without independent check on our individual attempts at calibration. I have been at great pains and expense to get our own instruments correctly standardised, but the results have not been entirely satisfactory. It is almost impossible for manufacturers working in what has been well named "the heavy electrical trades," for the reasons mentioned by Dr. Fleming, to arrange a satisfactory calibrating room at or near their works. Freedom from vibration and absence of disturbance of the magnetic readings is not obtainable; and, further

than that, we none of us have sufficient leisure for carrying out these calibrations in the way they should be carried out. Therefore, for purely commercial reasons, we manufacturers are fully prepared to assist in any reasonable scheme which will give us the independent check on standardising that we require, and I think that the best method of doing this will be to let this Society move in the matter. This Society is sufficiently representative of electrical engineering, and ought now to take the lead, and I hope that steps will be taken at this present meeting to give effect to the resolutions which the author has put at the end of his very valuable and interesting paper.

Mr. C. H. W. Biggs: You will excuse me, Sir, but I had written down a motion which bears upon the remarks that Mr. Crompton has just made, but I do not know whether the initiative should be taken by the meeting or the Council. The motion I had was to request the Council to form a committee to carry out the idea suggested by Dr. Fleming in his paper. Will you tell me whether the initiative should come from us or from you?

The President: I am much obliged to you, Mr. Biggs, for the suggestion, but it would be better, I think, if it comes from you. It will here receive a greater number of supporters than it could if it came from the Council itself; though before anything is proposed it will be well to have a little more discussion on the subject.

Mr. C. H. W. Biggs: Yes. It was only suggested by Mr. Crompton's remarks. The idea is floating in the minds of many others, I dare say.

Capt. H. R. Sankey, R.E.: I can only repeat what Capt. Cardew said, and observe that a laboratory for standardising instruments is very much required. A short time ago I felt the want of it myself very much, and it was only through the kindness of Dr. Fleming that I was able to standardise a couple of cells required for some experiments. If there had been in existence such a laboratory as is being discussed this evening I could have done what I wanted without having to trouble Dr. Fleming. I am sure he was very pleased to assist me.

Mr. W. H. Preece: It has always been a very favourite idea of mine that this Society should father an electrical laboratory,

more for the assistance of those of its members who have not laboratories themselves than for the purpose of establishing the means of standardising and verifying standards. I think there is a very broad distinction to be drawn between a laboratory for research and an office—or laboratory, if we may so call it—for verifying standards. As Dr. Fleming has very properly put it, to endow research is very likely to lead in time to an attempt to search for endowments; and not so much to assist research would I advocate a laboratory attached to this Society, but as a means of enabling those who have not apparatus themselves to fly to the assistance of instruments that will be of use to them.

Reference has been made to the go-aheadedness of the committee of the British Association, and it has been implied that they might have been a little hasty in arriving at their decision as regards the dimension of the ohm. But if there has been a committee which deserves well of this Society and of the world, it has been the committee of the British Association that established standards of electrical measurement. It placed the whole science upon a sound basis; it established a system of accurate measurement that has withstood all attacks for the last twenty years; and it has succeeded in producing a system of uniformity throughout the whole universe. Though the congress that met in Paris in 1881 showed, it is true, that the B.A. ohm was not absolutely what we now know to be the true ohm, still the measurements that were made twenty years ago were made by the ablest men of the day. The labours of that committee were guided by one of the greatest electricians that ever lived—Clerk Maxwell; they were carried out by one of the ablest secretaries that any committee ever had—Fleeming Jenkin; and they were supported by the names of men whom you see before you, and the highest in the scientific world.

It is true that the British Association unit happens to be about $1\frac{1}{3}$ per cent. wrong, and an effort was made by the Paris Congress in 1881, somewhat hastily, to remedy that wrong by establishing what has since been called the legal ohm; yet most unfortunately, we now know, or at least we believe we know, that even the decision arrived at in 1881 was in itself also wrong.

We are now in this unfortunate position, that, of the two units in use—the B.A. unit and the legal ohm—they are neither of them right. It has been a very difficult question to decide—Shall we in England adopt the legal ohm, or shall we be content to adhere to that which has served us so well for twenty years, viz., the B.A. unit ? and, after giving the most careful consideration from all points of view to the question, we have decided—when I say "we" I mean the Post Office—and in arriving at that decision we have been seconded by the cable companies and by all the colonies, and the result is that we have not adopted and are not going to adopt the legal ohm. The probability is that this decision will force a reopening of the question, and I have myself personally taken some steps in this direction. I have been in communication with Professor Rowland in America, and with Lord Rayleigh. Lord Rayleigh has been in communication with Wiedemann, and with two or three German physicists, and the conclusion that has been come to at the present moment is, that the time is scarcely ripe for reopening the question; but at any rate it remains in abeyance, and the probability is that, when we have seen the published details of Professor Rowland's operations, and when Weber and two or three other physicists on the Continent have succeeded in repeating their experiments, some other opportunity may occur to reopen the question, to form another congress, and to decide on what in future shall be the true ohm.

The great question before us to-night in reality is, How shall we proceed to carry out the views of Dr. Fleming as regards the establishment of some mode of verifying existing standards, whether they are right or whether they are wrong? and we have models before us to follow. We have the model of Greenwich; we have the model of the Board of Trade, that has been brought before us by Mr. Chaney; we have that of Kew, with all its work that has been so ably performed for so many years, and especially that portion of it which has been conducted by Mr. Whipple; and we have there something that we can follow. The operations at Kew have been conducted under the great care of a very able committee appointed by the Royal Society, who watch over all the operations conducted there; and it appears to me that if the

views of this meeting are met by carrying such a proposition as that mentioned by Mr. Biggs, it will ultimately lead to the formation of a committee of this Society that shall take up very much the same position as the committee of the Royal Society—a committee on standards, to watch over the operations, either of Kew, or of some laboratory established under the influence of the Board of Trade, or one directly under the influence of this Society. But I take it that Dr. Fleming has shown us unmistakably—and everybody at this meeting, by his applause, seems to agree with it—that the time has now come when we must take active and energetic measures to carry this out. But then comes the question of funds. It is all very well to talk about the hundreds of thousands of pounds that are flowing to the exchequer of the Patent Office, but some of us are aware that when money once gets into the hands of a Government department it is extremely difficult to get it out again. However, I am quite sure of this, that with the assistance of Mr. Chaney, of the Board of Trade; with the assistance of Dr. Jackson, of the Patent Office; and with all the power that other officers in other Government departments can bring to bear upon the matter, there can be little doubt that, if we take the proper course, by forming a committee and by going to work with a will, we shall succeed in getting funds to establish a laboratory; and I know that we shall succeed in getting a committee who will throw as much energy into the matter as do the committee of the Royal Society in regard to Kew.

Professor W. Grylls Adams: I had no intention, Sir, of making any remarks on the subject which Dr. Fleming has so clearly put before us. The question of the establishment of a laboratory for the purpose of testing measuring instruments, and certifying as to their correctness, is a very important one, and involves considerations which I fear Mr. Whipple has not fully taken into account when he suggests that this work should be taken up at the Kew Observatory. There is one point in connection with the value of units which I regard as of great importance. The practical units which we employ should be as nearly as possible equal to the theoretical values aimed at by the *British* Association Committee in their original determinations.

Some twenty-one years ago I had the honour to assist Professor J. Clerk Maxwell in the determination of the B.A. unit of resistance; but that unit is now found to differ so much from the theoretical value aimed at that it should be changed, and the legal ohm of the Paris Conference should be adopted. It may not be easy to change the old coils and make them new, but I would recommend that from a given date—say January 1, 1886—no new B.A. units should be made, but that from that time forward for all new coils we should adopt the legal ohm and the values of the units of electro-motive force and current which are consistent with the legal ohm. The French and Germans—and, indeed, most other nations—have shown their willingness to adopt the units which were fixed upon in Paris, and I feel sure that all practical electricians must see the importance of having the same electrical units throughout the world. Surely they should be the first to desire the adoption of the legal ohm, which is so near to the theoretical value that the only correction which can ever be required will be a statement that a set of coils is correct at a temperature of a few degrees from the temperature registered upon them.

Major-General WEBBER, C.B.: It has occurred to me, while listening to the able remarks of the last speaker, that something may be said as regards the urgency of this question. We should look to the fact that in the past, two standards of measurement came into existence, one might say, without really scientific authority of any kind, and that their existence has given a certain amount of trouble, not only to the industrial, but to the scientific world. One is, or was, the Birmingham wire gauge, with which a committee of this Society dealt about three years ago; and the members of that committee may remember, when they attended a meeting at the Board of Trade on the subject, that one of the greatest difficulties, or chief objections, with which they had to deal was that raised by manufacturers against making any alteration in the Birmingham and other well-known standards which they had been using for the purposes of manufacture. The other standard was the candle. Several of us can recall how much time was occupied at the Paris Congress in

1881 in discussing the value of the candle as a standard of measurement. Every one here probably knows how it was that the candl, as a standard of measurement, came into existence, and also that the only reason why it was clung to by some at that congress was that it was a popularly known standard, although a very inaccurate one. Those are two examples which appear to me to show strikingly that the sooner the means of standardising what is a well-known want of all electrical engineers is taken in hand the better, and the sooner shall we forestall the establishment, by custom, of inaccurate standards.

Mr. J. Munro: I did not intend to speak upon this matter, but I think that if such a laboratory is founded it should have a connection with this Society, as Professor Adams has said, and that it should be situated in London, and not at Kew, which appears rather out of the way for sudden requirements. There would be some difficulty in getting a good position in London, but somewhere in the parks or outskirts a quiet place could probably be obtained for the purpose. A committee of this Society ought to consider the matter, and I shall be glad to second the motion of Mr. Biggs when put to the meeting.

Professor W. E. Ayrton: Dr. Fleming's most able and interesting paper really embraces two distinct propositions. The one is a recommendation to organise a testing establishment; and the other, to create an electrical research laboratory. With the importance of the former—the establishment of some place where electrical apparatus can be tested—there can be no two opinions. Nothing would be more convenient than to be able to send any piece of electrical apparatus one wished to such a place and have it tested, not as a favour, but on the payment of some small fee; the officials at the place undertaking to test every kind of electrical apparatus—perhaps, however, confining "electrical brushes" to the collectors of dynamo machines. With the first part, then, of Dr. Fleming's proposal I am therefore thoroughly in accord. But with reference to the second part I venture to differ from him. I think it would be a mistake that we should assume, or in any way suggest that it should be recognised that *there* are no laboratories in this country for the carrying out of

electrical research. I would like to ask what is the main duty of the physical laboratories of the Universities. There are the funds; there certainly is the apparatus; there exist the men—the most able men in many cases, that the world can produce—and what more can be desired? In many cases a great deal of very valuable original work comes from these laboratories; but in others I must confess that hardly sufficient attention is given to original work; and this, it appears to me, arises from the fact that the time of the professors is in these cases to a great extent wrongly employed—not from their own fault, but because, instead of being allowed to instil into their students the enthusiasm for original research, the professors are required to train those students merely to pass examinations. We are at the present moment—the whole of Great Britain is at the present moment—cursed with a sort of Chinese mania for examinations. I can foresee to what that mania will lead, because I have seen Canton myself, where long ranges of examination cells, in which the examinees are separately imprisoned to prevent their copying, show one to what an absurdity this national examination system may be brought. The business of the professors in our Universities, as distinguished from the mere business of teachers at schools, should be to train students in research, and not to merely cram them up so that they may pour out a certain amount of cut-and-dry information on a certain day, and then forget it all as soon as they can. Well, then, I think that it would be a national misfortune if we were to assume, or ask for it to be recognised, that it was not the duty of these professors to carry out this research work, or to suggest that any other machinery was required for increasing our knowledge of unknown physical laws. These professors have the apparatus and scientific ability, and in many cases the means; what is solely wanted is the time, and possibly in some cases extra assistance. Therefore, as far as regards the second part of Dr. Fleming's paper, and the second part of his recommendation, I would venture to suggest his withholding that altogether, and merely recommending that there should be a national testing establishment for the verification of electrical apparatus.

Mr. A. Bernstein: Mr. President, I am surprised at your call, but being called upon I will venture to make a few remarks on the subject of Dr. Fleming's paper.

There can be no doubt that an insititute for standardising electrical instruments is not only desirable, but is almost a necessity at the present time, and I fully agree with Dr. Fleming on that question. But it seems to me that the difficulty under which we are labouring is not entirely obviated by the existence of the contemplated institute. A certain difficulty exists with electrical instruments which is not apparent to such a degree in other measuring apparatus.

If we send a two-foot rule to Kew, and the same is approved, we may consider it correct, and if it is made of proper material it will remain sufficiently correct for all practical purposes. If a piece is broken off, we know that our rule will no longer measure two feet. A similar remark applies to weights. In regard to thermometers, we are not so certain of their constancy, because the glass bulb seems subject to changes, which slightly affect the position of the zero point; but as these probable changes are known, they can be taken into account.

With electric instruments we are as yet in a very unfavourable position. We may have our instruments standardised to-day, but we know nothing as yet of their probable state after four weeks' use. All we are aware of at the present time is the fact that our instruments—I refer to the voltmeters and ampère-meters with direct reading—are more or less subject to changes. As we can never ensure perfect constancy, it would be desirable to know the amount of these changes, so as to get an idea of the probable error of our measurements.

Now it seems to me that the contemplated institute will prove very beneficial in this respect; because, by frequently comparing our instruments with standards, we shall become better acquainted with their peculiarities, and this will at the same time lead to the construction of superior instruments.

Mr. W. M. Mordey pointed out that Joule had many years ago used and described an instrument for the measurement of current, similar in general principles to that sketched on the

board, and called by Dr. Fleming "Lord Rayleigh's dynamometer." It was a zero instrument, in which the attraction between two parallel coils was balanced against a weight placed on a scale-beam. Mascart and Deprez had used a similar instrument in France. Deprez called it "Mascart's dynamometer." He asked whether it should not be called after Joule.

Mr. J. SWINBURNE: May I ask what steps have been taken with regard to the "Nomenclature" Committee of this Society which was formed a little while ago? Could that committee be amalgamated with the proposed one now under discussion?

The PRESIDENT: What committee do you refer to?

Mr. J. SWINBURNE: Some time ago a paper was read by Professor Jamieson, the result of which was the formation of a committee. I want to point out particularly that if another committee is formed to discuss the advisability of adopting or rejecting the new ohm, it should consist largely of those who make bridges or constantly use them. Such committees are too apt to be formed of men who are scientific only.

The PRESIDENT: I think that the proposition now under discussion is so totally different in character to the subject that the other committee was formed for, that it would be very much better to have a separate committee for it.

At the request of the President, Mr. BIGGS made the following proposition:—"That this meeting desires the Council of the Society to consider the best means by which the standardising of electrical apparatus can preferably be carried into effect, as suggested in the paper communicated by Dr. Fleming this evening;" and added: I consider that it would be leaving the whole idea in the hands of the Council, because I think that they should see and discuss for themselves the best means by which they could carry this into effect. It is almost impossible at a meeting of this kind to discuss ways and means. Various ideas have been thrown out which can be considered by the Council, and they will select the best men in their judgment.

Mr. J. MUNRO: I beg to second it.

The PRESIDENT then put the motion to the meeting, and it was carried unanimously.

The PRESIDENT called upon Dr. Fleming to reply to the discussion.

Dr. FLEMING: I must first, Sir, thank the Society very sincerely for the very kind way in which this paper has been received. My object was, not to dogmatically state any opinions of my own, but, by throwing out a few suggestions, to give opportunity to the Society to express its views upon the matter; and I may claim to have succeeded in my object, as the opinion of very many members has been expressed upon the subject. I fully concur with the remarks which fell from Professor Ayrton and others that any proposition for aiding quantitative research should not be included in a proposal for a standardising laboratory. What is wanted is to be able to send any electrical measuring instrument to some place to be checked against acknowledged standards, to get it done quickly, and to get it done for nothing if possible, or at least for as little as possible.

With respect to one criticism which has been made—to the effect that the British Association unit was adopted hastily—I think that anyone who has made himself acquainted with the amount of research and the experiments carried out by the very first authorities on this question, must be led to the conclusion that, although differing, as we now know, from the theoretical value aimed at, it represented the best that skill and industry could do at that time. Anyone who has, for instance, carefully studied Lord Rayleigh's papers, must rise from them with the conviction that it is no easy work to secure the result desired.

Coming back to the real question at issue, it is evident from what has been said that if adequate provision could be made for such standardising as is required, its convenience would be felt in all directions. As Mr. Crompton has well said, no manufacturer can undertake the work, or carry it on under the required conditions. I have not alluded particularly to the question of a standard of light, but, as General Webber has alluded to it, I may say that the standard I am using at present is the pear oil, or acetate of amyl, lamp, made by Messrs. Siemens & Co., and which, under certain conditions, is a great improvement on the standard candle.

In conclusion, whilst desiring very earnestly that provision should be made for some such standardising laboratory, the other work mentioned as possible alongside of it ought certainly to be left as proper for the existing physical laboratories; and there is no doubt that more quantitative research of this kind would be undertaken if laboratories were better provided with apparatus, and the professors in them relieved of a good deal of what Professor Ayrton has called the Chinese system of examinations, which occupies the time of those who are best able to conduct research in teaching the elements of practical physics to young gentlemen preparing for competitive examinations.

The PRESIDENT: Gentlemen, I will ask you to accord to Dr. Fleming a hearty vote of thanks for his most excellent paper. (Carried unanimously.)

I have the honour of announcing that the Annual General Meeting will take place on the 10th December next.

A ballot took place, at which the following were elected:—

Foreign Member:
Louis Maiche.

Associates:

Joseph Appleton.	William Henry Masters.
Arthur Cecil Curtis-Hayward.	Frederick William Neale.
Max Kotyra.	Frederic Charles Rowan.
William James Mackenzie.	John William Turner.

Students:

William Henry Collis.	T. G. M. Ladds.

The meeting then adjourned till December 10th, 1885.

The Fourteenth Annual General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 10th December, 1885—Mr. C. E. SPAGNOLETTI, M. Inst. C.E., President, in the chair.

The minutes of the previous meeting were read and confirmed.

The PRESIDENT announced that the ballot for the election of Members of Council and Officers would take place that evening, and that the ballot-box would remain open until 8.30 p.m.

Mr. J. Aylmer and Mr. Gordon Wigan, M.A., were appointed Scrutineers of the ballot.

The names of new members were announced and suspended.

The following transfers were announced as having been made, on the approval of the Council, from the class of Students to that of Associates:—

Mr. H. W. Ansell.	Mr. P. J. Gomez.
,, O. Burne.	,, C. V. Porter.
,, E. J. M. Clapham.	,, H. L. Webb.
,, H. J. Eck.	,, H. M. Window.

Donations to the Library were announced as having been received since the last meeting from Mr. H. M. Brunel and from the Institution of Civil Engineers.

The SECRETARY remarked that Mr. Brunel's donation, though comparatively small in one sense, possessed importance in another sense, seeing that the book presented was one of the early pamphlets of Sir William Fothergill Cooke on railway telegraphs, a copy of which was missing from the Ronald's historical collection. Mr. Brunel had heard of the incompleteness of the Society's Library in that respect, and had kindly presented a copy.

An interesting donation had also been received from M. Gaston Planté, through Mr. W. H. Preece, by whose good offices it had been secured.

Mr. W. H. PREECE: M. Planté, in the early part of 1884, sent to the Inventions Exhibition a specimen of engraving on

glass by his rheostatic machine, and also a picture of peculiar arborisations produced on sulphur by the same machine; and he wrote me, after the close of the Exhibition, to say that he would have great pleasure in presenting these specimens to the Society of Telegraph-Engineers, and, in his name, I have to-day presented them to the Council for the purpose of their suspension in the Library.

(The specimens were on view before the meeting.)

A vote of thanks was accorded to the donors for their presents.

The PRESIDENT remarked that a suitable letter of thanks would be addressed to M. Gaston Planté in acknowledgment of his specimens.

The SECRETARY read the following Report of the Council:—

REPORT OF THE COUNCIL.

The number of elections into the Society during the year somewhat exceeds that of 1884, and comprises 7 Foreign Members, 5 Members, 71 Associates, and 22 Students—total, 105.

Besides these, 14 candidates have been approved for ballot at our first meeting next month.

By deaths and resignations our losses have been as follow:—1 Honorary Member, 4 Foreign Members, 17 Members, 12 Associates, and 1 Student—total, 35.

Among the deaths we have to lament the loss of Dr. Edward Davy, whose early labours in connection with the application of electricity to telegraphic purposes were publicly recognised by the Society in electing him an Honorary Member last year. Mons. de St. Martial (Foreign Member), Mr. W. L. Ternant, General C. Douglas, R.A., General Sir P. Scratchley, R.E., and Professor Fleeming Jenkin—distinguished as one of the most advanced electricians and electrical engineers of our time, and some account of whose important labours was given in Part 57 of the *Journal*—have also been taken from us.

The Institution of Civil Engineers continues to extend to the Society the great privilege of holding its general meetings in their lecture hall.

It will be observed by the subjoined list of the papers read

before the Society during the session that the subjects of which they treat embrace both practice and theory, and that they are not confined to any one branch of electrical science.

LIST OF PAPERS READ BEFORE THE SOCIETY DURING THE YEAR 1885.

Date.	Title.	Author.
Feb. 12.	On some Experiments in Electrotyping with a Dynamo-electric Machine, carried out at the Ordnance Survey Office, Southampton	Capt. H. R. Sankey, R.E., Associate.
" 12.	The Working of Railway Signals and Points by Electro-Magnets, and Controlling them in conjunction with a Complete Block System Efficiently and Economically by a Current from a Primary or Secondary Battery	Illius A. Timmis, M. Inst. M.E.
Mar. 12.	On Constant Electro-motive Force in an Electric Light Circuit ...	Sir David Salomons, Bart., M.A., Member.
" 26.	On the Seat of the Electro-motive Forces in a Voltaic Cell ...	Prof. Oliver J. Lodge, D.Sc.
May 14.	Electrical Definitions, Nomenclature, and Notation	Prof. Andrew Jamieson, F.R.S.E., Member.
" 28.	Ship Lighting by Glow Lamps, embodying Results of Trial for Economy on H.M.S. "Colossus"	J. Farquharson, Member.
" 28.	Electric Lighting at the Forth Bridge Works	J. N. Shoolbred, B.A., M. Inst. C.E., Member.
June 11.	On the Calculation of Mains for the Distribution of Electricity	W. H. Snell, Associate.
Nov. 12.	The Telegraphs of the Nile Expedition	Major-Gen. Webber, C.B., (Ret.) R.E., Past-President.
" 26.	On the Necessity for a National Standardising Laboratory for Electrical Instruments ...	J. A. Fleming, M.A., D.Sc., Memb.
Dec. 10.	A Method of Eliminating the Effects of Earth and Polarisation Currents in Fault Testing	W. J. Murphy, Assistant Electrician, E. & S. A. T. Co.'s s.s. "Great Northern."
" 10.	A Method of Localising a Fault in a Cable by Tests from One End only	H. Kingsford, Associate.

In respect of those papers read during the year ending June 30th, and which were eligible for competition* for the Society's annual premium, the Council have made the following awards:—

The Society's Premium, value £10, to Professor Oliver Lodge, for his paper, "On the Seat of the Electro-motive Forces in a Voltaic Cell."

The Paris Electrical Exhibition Premium, value £5, to Mr. W. H. Snell, Associate, for his paper, "On the Calculation of Mains for the Distribution of Electricity."

The Fahie Premium, value £5, to Capt. H. R. Sankey, R.E., Associate, for his paper, "On some Experiments in Electrotyping with a Dynamo-electric Machine."

As announced at the Ordinary General Meeting of May 14th, the Council, looking to the importance of a uniform system of electrical nomenclature and notation, which formed the subject of Professor Jamieson's paper read on that evening, resolved to appoint a comprehensive and influential committee to consider it.

They have satisfaction in reporting that, in addition to the assistance of a large number of members of the Society, they have obtained the co-operation of nearly all the professors of physics at the universities and leading public colleges of the United Kingdom, as well as that of Mons. Blavier, the president, and Mons. E. Hospitalier, the secretary, of the French committee formed for a similar purpose.

The general committee appointed a sub-committee of its members to consider and report upon the best course to be pursued in dealing with the subject, and the latter have already made their recommendations as to the different heads under which the chief work to be undertaken should be done.

These recommendations have been approved and adopted by the general committee, who have appointed a sub-committee to carry out the important work therein indicated.

As reported to the General Meeting of February 12th, the Council unanimously passed the following resolution in reference

* Papers by Members of the Council are not eligible.

to the Electric Lighting Act, and forwarded a copy thereof to the President of the Board of Trade :—

> "*Resolved unanimously*—That the Society of Telegraph-Engineers and Electricians have seen with regret that since the passing of the Electric Lighting Act of 1882 public lighting by electricity has not made that advance in this country which was confidently anticipated, and hence there has not been that stimulus to progress in many branches of electrical science which might have been expected, judging from the history of submarine telegraphy.
>
> "That the Society, having had the matter under careful consideration, have come to the conclusion that the restrictions in the said Electric Lighting Act have in a great measure contributed to check the advance of public lighting by electricity; and they venture to request the President of the Board of Trade to consider whether the said restrictions may not be modified or removed."

The Council are aware that equally strong representations upon the subject were made to the Board of Trade from other quarters, and they confidently hope that in the next Parliament the matter will obtain that attention which it deserves, and that such amendments in the Act will be made as to render it a help, rather than an obstruction, as it now is, to the development of public and domestic lighting by electricity.

The importance of the subject treated of by Dr. Fleming in his paper read on November 26th, namely, the necessity of a national standardising laboratory for electrical instruments, was at once recognised by the meeting, which passed a resolution requesting the Council to consider the best means of carrying into effect the suggestions contained in Dr. Fleming's paper.

The matter will accordingly receive the careful attention of your Council.

The Society's Library, as will be seen by the Librarian's *Report*, continues to receive important accessions through the

liberality of not only our own members, but of other writers on electrical subjects.

In June last, upon the invitation of the President, who has for so many years been intimately connected with the Great Western Railway Company, the members enjoyed a most agreeable visit to the comprehensive engineering works of that company at Swindon, the machinery in the various branches of the vast establishment being most courteously and ably explained by the members of the staff of the locomotive department.

Although the Annual Balance Sheet can only be made up after the close of the year, the Honorary Treasurer is enabled to report that the financial position of the Society is satisfactory; a further amount of £120 on account of life compositions has been invested during the year.

The subscribers to the Guarantee Fund of the Vienna Electrical Exhibition very liberally presented the surplus of the fund, amounting to £21 18s. 10d., to the Society for the purposes of the Library, or towards forming the nucleus of a fund for establishing a Laboratory.

Members will have observed that the form of balloting list for the Annual Election has been amended, and that the number of members and associates recommended as eligible to fill the vacancies on the Council is double that of the vacancies.

LIBRARIAN'S REPORT.

F. H. WEBB, Esq., *December 8th*, 1885.

Secretary,

Society of Telegraph-Engineers and Electricians.

DEAR SIR,

I have the pleasure to hand you, for the information of the Council, my Sixth Annual Report on the Library of the Society.

The year 1885 has been a somewhat uneventful year in the annals of electricity. There have been no very great discoveries, and nothing much beyond great depression of trade, to occupy the minds of electrical engineers; and it is the latter circumstance, I believe, which will account for the slight falling off in the

number of those who have referred to the treasures which are contained in the Society's Library. The number of those who have signed the visitors' book amounts to 461, of which 167 are non-members.

The collection of the Electrical Specifications of Patents continues to form an important duty in the Library. These have been carefully kept up during the year, and those recently published have been available for reference week by week. The activity which was displayed in the Patent Office on the inauguration of the new Act of Parliament, and during the first year of its operation, has not been kept up, and there has been a marked falling off in the number of applications for patents, although the number of those having reference to electricity has been very nearly up to the average. The number of applications up to December 4th amounted to 14,872, against 15,819 for the eleven months ending December 1st, 1884. The applications relating to electricity amount to 1,048, or 7·04 per cent. The following table will show the number of electrical patents which have been applied for during the past six years:—

1880.	1881.	1882.	1883.	1884.	1885.
260	450	845	620	1,095	1,048

As far as I have up to the present been able to complete my figures, I find that out of the 1,048 applications for electrical patents just over 800 had been accepted up to the 1st December.

I am glad to be able to report that H.M. Commissioners of Patents acceded to an application which I made to them for the *Illustrated Journal of Patented Inventions*, and a complete set has been presented, and we receive the numbers as they are published. This journal, which is similar to the journal published by the United States Patent Office, is the greatest advancement which has been made by H.M. Commissioners, as it contains well-illustrated abstracts of the published specifications.

All the published abridgments of patents relating to electricity are now in the Library, and the thanks of the Society are again due to H.M. Commissioners of Patents, and to Mr. R. Morris, the Superintendent of the Store Department, for the prompt manner

in which my applications have been acceded to, and for the assistance which we receive in obtaining the specifications as soon as published.

The additions to the Library during the year have been very numerous, and number nearly 260 separate works, many of considerable value and importance. A catalogue of the more recent accessions is appended hereto.

Arrangements have been made to exchange the Society's journal for several new publications, and appended hereto is a list of the periodicals received by the Society.

I have not the pleasure to announce any large presentations such as those the Library received last year from Lady Siemens and Mr. J. G. Symons and others, and I would venture again to draw the attention of the members to the desirability of adding some of the large standard scientific works, which are beyond the present means at the disposal of the Society.

During the year we have made considerable progress towards perfecting the Society's Library by adding a large number of English and foreign works which we did not possess, and in the lists of accessions which have been published from time to time in the Society's journal will be found a large number of French and German works which have been purchased subject to large discounts, and as far as the limited vote at the disposal of the Library would allow.

The increased labour in compiling the lists and keeping up the collection of electrical specifications, and other reasons, have prevented me from being able to report the completion of the classified catalogue of electrical works which I have in hand, and which I have before referred to. The cards are all completed and nearly all roughly classified, and are to a certain extent available for reference. The great percentage of foreign titles, however, and the difficulty of properly classifying them, is a work of such magnitude that it will be some time before I shall be able to report it as thoroughly completed. It has been already found of considerable value in the Library, and will, when completed, I venture to hope, be well worthy of the Society.

I beg again to draw attention to the want of shelf space in

the Library, and it has been necessary during the year to stow away a large number of books in parcels, in order to find room on the shelves for current publications, and for those works which are more likely to be referred to.

I am,

Dear Sir,

Yours faithfully,

A. J. FROST,
Librarian.

APPENDIX TO LIBRARIAN'S REPORT.

LIST OF PERIODICALS RECEIVED BY THE SOCIETY.

ENGLISH.

Asiatic Society of Bengal, Journal and Proceedings.
Cambridge Philosophical Society, Proceedings.
Electrical Engineer.
Electrician.
Engineer.
Engineering.
English Mechanic and World of Science.
Greenwich Magnetical and Meteorological Observations.
Illustrated Journal of Patented Inventions.
Incorporated Law Society Calendar.
Institute of Patent Agents, Transactions.
Institution of Civil Engineers, Proceedings.
Institution of Mechanical Engineers, Proceedings.
Iron and Steel Institute, Proceedings.
Journal of Science.
Military Telegraph Bulletin.
Nature.
Patents' Journal, Commissioners of.
Philosophical Magazine.
Physical Society, Proceedings.
Royal Dublin Society, Transactions and Proceedings.
Royal Engineers' Institute, Proceedings.
Royal Institution, Proceedings.
Royal Meteorological Society, Proceedings.
Royal United Service Institution, Proceedings.
Society of Arts' Journal.
Society of Engineers, Proceedings.
Telegraphic Journal and Electrical Review.
Telegraphist.
University College Calendar.

AMERICAN.

American Academy of Science and Arts, Proceedings.
Electrical Review.
Electrical World.
Electrician and Electrical Engineer.
Franklin Institute, Journal of.
John Hopkins University Calendar.
Journal of the Telegraph.
Library of Cornell University.
Ordnance Department of the United States, Notes.
Science.
Scientific American.
Smithsonian Institution Reports.
United States Patent Office, Official Gazette of.

FRENCH.

Annales Télégraphiques.
Bulletin de la Société Scientifique Industrielle de Marseille.
Cosmos les Mondes.
Journal de Physique.
Journal du Gaz et de l'Electricité.
Journal Télégraphique.
La Lumière Electrique.
Le Franklin.
L'Electricité.
L'Electricien.
Revue Internationale de l'Electricité et de ses Applications.
Société Belge d'Electriciens, Bulletin de la.
Société Française de Physique, Seances de la.
Société des Ingénieurs Civils, Mémoires.
Société Internationale des Electriciens, Bulletin de la.

GERMAN.

Annalen der Physik und Chemie.
Beiblätter zu den Annalen der Physik und Chemie.
Centralblatt für Elektrotechnik.
Der Elektro Techniker.
Elektrotechnische Zeitschrift.
Electro-technischer Anzeiger.
Repertorium für Experimental-Physik für Physikalische-Tecknik.
Verhandlungen des Vereins zur Beforderung des Gewerbfleisses.
Zeitschrift für Elektrotechnik.
Zeitschrift für Instrumentenkunde.

ITALIAN.

Government Telegraph Department, Annual Report.
Il Telegrafista.

JAPAN.

Annual Report of Telegraph Department.

RUSSIAN.

Government Telegraph Department, Annual Report.

SPANISH.

La Electricidád.

The PRESIDENT proposed—"That the Report of the Council be received and adopted, and that the same be printed in the Proceedings of the Society."

Professor W. G. ADAMS seconded the proposition, which was carried unanimously.

Professor J. PERRY proposed—"That the cordial thanks of the Society be presented to the President, Council, and Members of the Institution of Civil Engineers for so kindly and liberally continuing to permit the Society to hold its general meetings in the theatre of the Institution." He thought that a proposal of that kind needed no special words to commend it to the Society of Telegraph-Engineers. They had only to look round the room and see the busts and portraits to understand that they were in a room which had seen the presence of all the great civil engineers of the century—in fact, all those men who had done so much for Anglo-Saxon civilisation. The Institution of Civil Engineers deserved their best thanks for the way in which they tried to carry out the object with which their Institution was founded, namely, for civil engineering generally. The Society's branch of civil engineering—electrical engineering—had been nursed by the Institution of Civil Engineers into the state in which it was then found, and if signs presented themselves that their branch might overpower other branches of civil engineering, that Institution deserved thanks all the more for having supported it from the beginning.

Mr. E. GRAVES seconded the proposition, which was carried most heartily.

The ballot-box was withdrawn.

The PRESIDENT proposed a hearty vote of thanks to the Honorary Foreign Secretaries and Treasurers. Good working foreign secretaries materially helped and benefited the Society, and those gentlemen who at present gave their services really rendered very material support to the progress and welfare of the Society.

Mr. F. H. WEBB rose as an associate, and seconded the resolution. He did so because his official position as Secretary enabled *him*, perhaps better than anyone else, to bear testimony to the

work which the Honorary Foreign Secretaries and Treasurers performed in the interest of the Society. He was quite sure that in many cases the work must be very considerable. Some of the secretariats comprised a large number of members, and the distribution of the journals to those members, the collection of their subscriptions, and the correspondence which frequently became necessary between himself and the local secretaries, entailed much trouble on them—far more than in former years—and he felt that it was only right that the meeting should be made acquainted with these facts.

The motion was carried unanimously.

Professor J. A. FLEMING proposed a very hearty vote of thanks to the Honorary Treasurer, Mr. E. Graves, for his services to the Society during the past year. He had great pleasure in doing so. The members had just learnt by the Report of the Council that their Society was in a satisfactory financial condition, while it was often found with societies and corporations that the result of a year's working showed a balance due to the treasurer; and seeing the satisfactory condition of the Society, a vote of thanks was the least that could be offered to its Honorary Treasurer.

Mr. R. E. CROMPTON seconded the proposition, which was carried unanimously.

Mr. EDWARD GRAVES acknowledged the vote, and remarked that during the past year nothing had specially affected the particular duties that he had undertaken to perform. He supported the observation as to the satisfactory financial position of the Society, but added that it would be still more so if the amount always standing to unpaid arrears were considerably lessened: it was to some extent a paper credit.

Mr. DESMOND G. FITZGERALD moved that the thanks of the Society be presented to Mr. J. Wagstaff Blundell and Mr. Frederick C. Danvers for their kind services as Auditors of the Society. He thought those gentlemen thoroughly deserved a cordial vote of thanks for their services, which were no doubt of a very dry nature.

Professor D. E. HUGHES seconded the motion, which was unanimously carried.

Mr. ALEXANDER SIEMENS moved that the thanks of the Society

be presented to Messrs. Wilson, Bristows, & Carpmael, the Honorary Solicitors, for their kind and valuable services rendered to the Society, through G. L. Bristow, Esq. Every one knew that it was impossible to get through life without a solicitor, and he thought the Society was very much to be congratulated upon having secured the services of so eminent a firm at so reasonable an understanding. It was to be hoped that since the great question of the registration of the Society had been successfully negotiated by them their labours would not be great; still it was satisfactory to know that such a good firm was ready to help if necessity arose.

Mr. A. Stroh seconded the motion, which was carried unanimously.

The following paper was read :—

A METHOD OF ELIMINATING THE EFFECTS OF EARTH AND POLARISATION CURRENTS IN FAULT TESTING.

By Walter J. Murphy, Assistant Electrician, E. & S. A. T. Co.'s S.S. "Great Northern."

The Secretary then read the following paper :—

A METHOD OF LOCALISING A FAULT IN A CABLE BY TESTS FROM ONE END ONLY.

By H. Kingsford, Associate.

In experimenting some years ago to discover a trustworthy method of localising a partial fault in the case of a laid cable where no return wire was available, unsatisfactory results were nearly always obtained from the application of "Blavier's tests" pure and simple.

I was then led to investigate for the first time a formula which until then had never interested me; total severances of the cable, or faults when both ends were available, having so far been the only defects which had occupied my attention. It needed, of course, but little consideration to trace the source of error, as testing to breaks had assured me of the well-known fact

that the resistance of an end varies (as a rule considerably) in some ratio to the current through it, decreasing, with increased electro-motive force, until a minimum resistance has been obtained. A partial earth would, of course, be affected in the same way by the so-called "polarisation." To obviate the error due to this fact, I devised a method which I shall presently state, and an account of which I have long intended to publish. It is most probable that I should have delayed longer, being anxious to publish at the same time other matter which I have not yet had time to work into shape; but I was reminded of my intention by reading a very interesting article by Messrs. J. Anderson and A. E. Kennelly in the *Electrician* of July 17th, 1885. These gentlemen recognise the source of error which I mention as accompanying the simple "Blavier's test," and suggest for its elimination a test the main principle of which is the same as that which characterises my own modification of that method. For reasons, however, which will, I think, be considered by most electricians as sufficient, I can but think that my method may in some cases be preferable. That of Messrs. Anderson and Kennelly requires an observer, battery, and a set of instruments at each end of the cable—a mode of testing which would be apt at times, I consider, to involve some little anxiety at least on the part of the responsible person. My method requires tests from one end only, and is as follows:—

In order that the current through the fault may be the same when the distant end is free as when it is to earth, proceed thus: First, by taking the ordinary "Blavier's test," approximate to distance of fault, and let that approximation from testing station $= x$ in ohms; from that we obtain approximate resistance from the fault to the distant end $- y$ ohms $-$, and approximate resistance of fault $- z$ ohms $-$. With these data we go to work to obtain an approximation to the resistance which must be inserted at the home end of the line when testing cable free at distant end. Should the test be taken on shore, there is perhaps no better method to adopt than that known as reproduced deflection. Using this method,

Let r = resistance of battery;

„ r' = resistance of well-shunted galvanometer, or $\dfrac{g\,s}{g+s}$

„ a = E.M.F. of battery.

Then $\dfrac{a}{r + r' + x + z} = \dfrac{a}{f}$ = current through fault when line is free at distant end;

and $\dfrac{a}{r + r' + x + \dfrac{y\,z}{y+z}} = \dfrac{a}{g}$ = current through x when line is to earth at distant end.

Portion of $\dfrac{a}{g}$ flowing through fault = roughly $\dfrac{\dfrac{a\,y}{g}}{y + z}$

Let this quantity $= \dfrac{a}{h}$, then $h - f$ will represent the first approximation for resistance to insert, in ohms, or R_1.

Then, placing R_1 in circuit, we now take another observation with the line free at distant end, and then test again from the actual end of cable, with the distant end to earth. Thus, in the same manner as we procured x, y, z, and R_1, we obtain x_1, y_1, z_1, and R_2, and so on until we find that $R_n = R_{n-1}$. Results obtained with this insertion will be correct if the test be carefully taken.

On board ship I prefer to use a Wheatstone bridge, and shall consider a case in which an even bridge is employed. Should it be deemed advisable to use a dividing bridge, the necessary slight alteration in formula will, of course, suggest itself.

We first take the simple "Blavier's test" as before, and obtain from the same, approximations x, y, and z.

Let a = E.M.F. of battery;

„ r = resistance of battery;

„ r'' = „ in either side of bridge;

„ R, R_a, R_b, &c. = resistance unplugged when equilibrium is obtained with distant end free;

„ R', R'_a, R'_b, &c. = resistance unplugged when equilibrium is obtained with distant end to earth;

„ R_1, R_2, R_3, &c. = first, second, third, &c., approximations in ohms to resistance which must be inserted when testing line free at distant end, R_n being insertion.

Then, when distant end is free,

$$\frac{1}{2} \cdot \frac{a}{r + \frac{r'' + R}{2}} = \frac{a}{2\,c} = \text{ current through fault;}$$

and when distant end is to earth,

$$\frac{1}{2} \cdot \frac{a}{r + \frac{r'' + R'}{2}} = \frac{a}{2\,d} = \text{ current through "}x\text{."}$$

In the same manner as before we now find quantity of $\frac{a}{2\,d}$ flowing through fault. Let this quantity $= \frac{a}{2\,e}$, then R_1 will equal $2\,(e - c)$.

Having now obtained R_1, we proceed as before to obtain x', y', z', always testing, of course, from the actual end of cable when distant end is to earth. We must proceed until insertion represented by $R_n =$ that represented by R_{n-1}.

I may hint with possible advantage to some readers that it is an excellent plan to make up a Wheatstone bridge of cable when practicable, especially in cases when the resistance to the fault is small, and when the fault is of such a nature as to require the application of a powerful electro-motive force.

In cases where the fault is considerably nearer to one end, tests should be taken from that end if possible. In the case of a high resistance fault near the distant end of the line, R_1, R_2, &c., might at times be so high as to seriously interfere with the sensitiveness of the test; in such cases the E.M.F. must be altered.

Should we find that additional inserted resistance makes no difference in result, we shall be safe, of course, in accepting such result as correct. As in every instance in which I have employed this method it has given me great satisfaction, I now publish it in the hope that others may find it as useful to them as it has proved to me.

At the conclusion, a vote of thanks was proposed and carried by acclamation.

The Scrutineers handed the result of the ballot to the PRESIDENT, who read the following list:—

President:

Professor D. E. HUGHES, F.R.S.

Vice-Presidents:

Sir CHARLES BRIGHT, M. Inst. C.E.
EDWARD GRAVES.
Colonel Sir FRANCIS BOLTON.
Dr. J. HOPKINSON, M.A., F.R.S.

Members of Council:

W. T. ANSELL.
Professor W. E. AYRTON, F.R.S.
E. B. BRIGHT, M. Inst. C.E.
Capt. PHILIP CARDEW, R.E.
R. E. CROMPTON, M. Inst. C.E.
WILLIAM CROOKES, F.R.S.
Prof. J. A. FLEMING, M.A., D.Sc.
Professor G. FORBES, M.A., F.R.S.E.
Professor JOHN PERRY, F.R.S.
ALEXANDER SIEMENS.
AUGUSTUS STROH.
Professor SILVANUS P. THOMPSON, B.A., D.Sc.

Associate Members of Council:

W. LANT CARPENTER, B.A., B.Sc.
GISBERT KAPP.
JOHN MUNRO.

Honorary Treasurer:

EDWARD GRAVES, V.-P.

Honorary Secretary:

Colonel Sir FRANCIS BOLTON.

Auditors:

J. WAGSTAFF BLUNDELL.
FREDERICK C. DANVERS.

Honorary Solicitors:

WILSON, BRISTOWS, & CARPMAEL.

The PRESIDENT: I propose that a hearty vote of thanks be given to the Scrutineers for their services. They have had a great deal of work to do as the result of the new form of ballot, and no doubt next year steps must be taken to render assistance to those gentlemen who perform the duties on the next occasion. I am informed that to do the work expeditiously four scrutineers and two clerks to assist them are required, so great is the interest taken in the ballot for Members of Council.

The vote of thanks to the Scrutineers was carried unanimously.

The meeting adjourned till Thursday, January 28th, 1886.

THE LIBRARY.

ACCESSIONS TO THE LIBRARY FROM MAY 1 TO NOV. 28, 1885.

BY ALFRED J. FROST, *Librarian.*

(*Works marked thus (*) have been purchased.*)

IT IS PARTICULARLY DESIRABLE THAT MEMBERS SHOULD PRESENT COPIES OF THEIR WORKS TO THE LIBRARY AS SOON AS POSSIBLE AFTER PUBLICATION.

America. [*Vide* United States.]

American Academy of Arts and Sciences. Proceedings. New Series, Vol. XII. (Whole Series, Vol. XX.) From May, 1884, to May, 1885. 8vo. 558 pp. *Boston*, 1885

Army Telegraphs. [*Vide* War Office.]

Asiatic Society of Bengal. Centenary Review of the Society from 1784 to 1883. 8vo. *Calcutta*, 1885

Bailey [Alderman W. H.] Ancient Telegraphs, from the fall of Troy to the Battle of Waterloo. 8vo. 21 pp. [Read before the Manchester Association of Employers, &c., 11th October, 1884.] *Manchester*, 1884
[Presented by Col. Sir Francis Bolton, Vice-President and Hon. Sec.]

* **Baker** [W. G.] Magnetism and Electricity. A specific subject of instruction in public elementary schools. 12mo. 144 pp. *London*, 1885

Bashforth. Tables for Calculating Remaining Velocities in Experiments with Small Shot. [See Paper by Major W. McClintock, R.A., *Ordnance Notes*, No. 321, Oct. 27, 1883.] *Washington*, 1885

Benét [S. V.] [*Vide* U.S.A. Ordnance Department.]

* **Bichat** [M. E.] **et Blondlot** [M. R.] Introduction à l'étude de l'Électricité Statique. 8vo. 141 pp. *Paris*, 1885

Bidwell [Shelford], M.A., LL.B. The Effect of Temperature on the Electrical Resistance of Mixtures of Sulphur and Carbon. 8vo. 7 pp. [*Phil. Mag.*, May, p. 347.] *London*, 1882

—— On the Electrical Resistance of Carbon Contacts. 8vo. 18 pp. [*Proc. Roy. Soc.*, No. 224.] *London*, 1883

—— On a Method of Measuring Electrical Resistances with a Constant Current. 8vo. 3 pp. [*Phil. Mag.*, May, p. 316.] *London*, 1883

—— On some Experiments illustrating an Explanation of Hall's Phenomenon. 8vo. 17 pp. Plate. [*Phil. Mag.*, April, p. 249.] *London*, 1884

—— On a Relation between the Coefficient of the Thomson Effect and certain other Physical Properties of Metals. 8vo. 4 pp. [*Proc. Roy. Soc.*, No. 232, p. 25.] *London*, 1884

—— On the Sensitiveness of Selenium to Light, and the Development of a similar Property in Sulphur. 8vo. 14 pp. [*Phil. Mag.*, Aug., p. 178.] *London*, 1885

Bidwell [Shelford], M.A., LL.B. On the Changes produced by Magnetisation in the Length of Rods of Iron, Steel, and Nickel. 8vo. 5 pp. [*Proc. Roy. Soc.*, No. 237p. 265.] *London*, 1885

Blavier [E. E.] Influence des Orages sur les Lignes Souterraines. 8vo. 8 pp. [Ext. des *Annales Télégraphiques*, Mai—Juin, 1885.] *Paris*, 1885
[Presented by J. Aylmer, Esq., Local Hon. Sec., Paris.]

Blondlot. [*Vide* Bichat et Blondlot.]

Bottomley [W.] [*Vide* Collet.]

* **Bottone** [S. R.] The Dynamo: How Made and How Used. 8vo. 73 pp. *London*, 1884

Boulengé. Chronograph: Its Use for obtaining Observed Velocities in Experiments with Small Shot. [See Paper by Major W. McClintock, R.A., *Ordnance Notes*, No. 321, Oct. 27, 1883.] *Washington*, 1885

Cambridge Philosophical Society. Proceedings. Vol. V., Part IV. *Cambridge*, 1885

* **Cantoni** [Giovanni] Relazioni fra la Polarizzazione Ellettrica dei Coibenti e la Polarizzazione dei Magneti. 4to. 30 pp. Plate. [Reale Accademia dei Lincei, Anno CCLXXX., 1882-83.] *Roma*, 1883

Christie [W. H. M.], M.A., F.R.S. [*Vide* Astronomer Royal.]

——— [*Vide* Royal Observatory, Greenwich.]

Clark [Latimer]. Transit Tables for 1886. Giving the Greenwich mean time of transit of the sun and of certain stars for every day in year; with an ephemeris of the sun, moon, and planets. 8vo. 71 pp. [Received Nov., 1885.] *London*, 1886

Clauson-Thue [W.] The A B C Universal Commercial Electric Telegraphic Code. Specially adapted for the use of financiers, merchants, shipowners, brokers, agents, &c. 8vo. 480 pp. 4th Edition. *London*, 1885

* **Collet** [Lieut.] Practical Guide for Compensation of Compasses without Bearings. Translated by W. Bottomley. With a Preface by Sir William Thomson, F.R.S. 8vo. 65 pp. *Portsmouth*, 1885

Crawford and Balcarres [Right Hon. The Earl of]. Dun Echt Observatory Publications. Vol. III. Mauritius Expedition, 1874. Division II., Determinations of Longitude and Latitude. 2 plates. La. 4to. 519 pp. *Dun Echt, Aberdeen*, 1885

* **Culley** [R. S.] A Handbook of Practical Telegraphy. 8th Edition. 442 pp. 8vo. *London*, 1885

Davis. [*Vide* Maver and Davis.]

Davison [Charles], B.A. On a Possible Cause of the Disturbance of Magnetic Compass Needles during Earthquakes. 8vo. 2 pp. [*Geological Mag.*, May, p. 210.] *London*, 1885

Delaurier [E.] Pile Régénérable. 8vo. 3 pp. *Paris*, 1885 (?)

Eden Fisher & Co. Prospectus of Telegraph Codes. 8vo. 8 pp. *London*, 1885

Fisher. [*Vide* Eden Fisher & Co.]

* **Fontaine** [Hippolyte]. Transmissions Électriques Renseignements Pratiques. 8vo. 80 pp. *Paris*, 1885

Franklin Institute. On the Efficiency and Duration of Incandescent Electric Lamps. 8vo. 127 pp. Report of a Special Committee appointed by the President of the Franklin Institute, in conformity with a Resolution of the Board of Managers, passed Nov. 12, 1885 (*sic*). *Philadelphia*, 1885

——— Journal of. No. 717, Vol. CXX. (Third Series, Vol. XC., No. 3.) Sept. 8vo. *Philadelphia*, 1885

Giornale del Genio Civile. Nos. 3, 4, Marzo—Aprile, 1885, Anno XXII. Serie Quarta, Vol. V. *Roma*, 1885

Grief [J. B.] Anleitung zur Errichtung und Instandhaltung oberirdischer Telegraphen und Telephon-Linien aus Lazare Weiller's Patent Silicium-Bronze-Draht. 12mo. 128 pp. *Wien*, 1885
[Presented by Rudolph Howard Krause, Associate.]

Hall. [*Vide* Bidwell.]

* **Hepworth** [T. C.] The Electric Light: Its Past History and Present Position. 8vo. 127 pp. *London*, 1884

* **Holthof** [F.] Die Technische Verwertung der Elektrizität. 8vo. 59 pp. *Halle a S.*, 1884

Hospitalier [E.] The Modern Applications of Electricity. Vol. I. Electric Generators—Electric Light. Second Edition. Translated by Julius Maier, Ph.D. 8vo. 496 pp. *London*, 1883
[Presented by Julius Maier, Esq., Ph.D.]

* ——— Domestic Electricity for Amateurs. Translated from the French, with additions, by C. J. Wharton. 8vo. 229 pp. *London*, 1885

——— Formulaire Pratique de l'Électricien. Troisiéme Année, 1885. 12mo. 309 pp. *Paris*, 1885

——— Sur la Mesure des Courants Redressés. 4to. 2 pp. *Paris*, 1885
[Presented by J. Aylmer, Esq., Local Hon. Sec., Paris.]

Institute of Patent Agents. Transactions. Vol. III. Session 1884-85. 8vo. 224 pp. *London*, 1885

Institution of Civil Engineers. Minutes of Proceedings. Vol. LXXX. 8vo. 484 pp. *London*, 1885

——— Minutes of Proceedings. Vol. LXXXI. 8vo. 475 pp. Plates. *London*, 1885

——— Minutes of Proceedings. With other selected and abstracted Papers. Vol. LXXXII. 8vo. 480 pp. Plates. *London*, 1885

——— Charter, Bye-Laws, and List of Members. 8vo. 140 pp. *London*, 1885

——— General Index to Proceedings, 1874-1884. 8vo. 247 pp. *London*, 1885

——— Brief Subject Index to Minutes of Proceedings, Vols. LIX. to LXXXII., Sessions 1879-80 to 1884-85. 8vo. 20 pp. *London*, 1885

——— HEAT in its Mechanical Applications. A Series of Lectures delivered at the Institution, Session 1883-84. 8vo. 236 pp. Plates. *London*, 1885

Institution of Mechanical Engineers. Proceedings, Lincoln Meeting, August, 1885. 8vo. 158 pp. [303 to 460 pp.] Plates. *London*, 1885

Iron and Steel Institute. Journal of. No. I. 1885. 8vo. 368 pp. *London*, 1885

Italy. [*Vide* Giornale del Genio Civile.]

* **Juppont** [P.] L'Électricité à l'Exposition de l'Observatoire de Paris. Applications Nouvelles—Progrès Récents—Actualités Électriques. 8vo. 141 pp. [*Publications du Génie Civil.*] *Paris*, 1885

Kew Observatory. Report of the Kew Committee for the year ending Oct. 31, 1884; with Appendices containing results of magnetical, meteorological, and solar observations made at the Observatory. 8vo. 24 pp. [*Proc. Roy. Soc.*] *London*, 1884

McClintock [Major W.], R.A. Experiments with Small Shot. [*Ordnance Notes*, No. 321, Oct. 27, 1883.] 4to. 17 pp. Washington, 1885

McCollough. Army Telegraphs. English, Morse, and Arabic Alphabets, &c., for the use of the Military and Egyptian Clerks, during the Egyptian Campaign. Fo. 1 p. *Cairo*, 1885
[Presented by Major-Gen. C. E. Webber, R.E., C.B., Past-President.]

Madsen [C. L.] Om Forskjellige Ledningssystemer i Store Telefonanlœg. [*On different systems of conductors in large telephone establishments.*] 8vo. 21 pp. *Kjobenhavn*, 1885

Manchester Association of Employers. *Foremen and Draughtsmen of the Mechanical Trades of Great Britain.* Report of Visit to and Inspection of Mr. M. Holroyd Smith's Patent Electrical Car and Line for Tramway Purposes at Cornbrook, near Manchester, 4th Oct., 1884. 8vo. 16 pp. *Manchester*, 1885

* **Maver, jr.** [William], **and Davis** [Minor M.] The Quadruplex. With chapters on the Dynamo-electric Machine in relation to the Quadruplex; the Practical Working of the Quadruplex; Telegraph Repeaters; and the Wheatstone Automatic Telegraph. By Wm. Maver, jr. 8vo. 128 pp. *New York*, 1885

Military Telegraphs. [*Vide* War Office.]

* **Moser** [Dr. James.] Electrostatic Investigations, especially relating to the Division of Induction in the Differential Inductometer and in the Electrophorus. 8vo. 14 pp. [*Proc. Roy. Soc.*, Vol. IV., Part IV., p. 309.] *London*, 1885

Mourlon [Charles]. Électricité à l'Exposition Universelle d'Anvers. 8vo. 72 pp. *Bruxelles*, 1885

—— L'Électricité à l'Exposition Universelle d'Anvers. Troisième Livraison. Septembre. 8vo. 36 pp. [pp. 103 to 108]. *Bruxelles*, 1885

* **Murphy** [P.] Rudiments of the Primary Forces of Gravity, Magnetism, and Electricity in their Agency on the Heavenly Bodies. 8vo. 513 pp. *London*, 1830

Ordnance Dept., U.S.A. [*Vide* United States.]

Patent Agents. [*Vide* Institute of Patent Agents.]

Patent Office. Abridgments of Specifications relating to Plating or Coating Metals with Metals. Parts I. and II. 1637–1866. 8vo. 198 + 99 pp. *London*, 1862-72

—— Abridgments of Specifications relating to Optical, Mathematical, and other Philosophical Instruments, including Nautical, Astronomical, and Meteorological Instruments. 1636–1866. 8vo. 552 pp. *London*, 1875.

Postmaster-General. Thirtieth Report on the Post Office. 8vo. 59 pp. *London*, 1884

—— Thirty-First Report on the Post Office. 8vo. 63 pp. *London*, 1885
[Presented by W. H. Preece, Esq., F.R.S., Past-President.]

Radiguet. [*Vide* Tommasi et Radiguet.]

Revue Internationale de l'Electricité. Monthly. *Paris*, 1885

* **Reynier** [Emile]. Piles Électriques et Accumulateurs. Recherches techniques. 8vo. 207 pp. *Paris*, 1884

Ronalds [Francis]. Copy of Correspondence relating to Kew Observatory, from Nov., 1842, to July, 1846. 4to. MSS. *London*, 1842-46

Rowell [G. A.] A Lecture on the Storm in Wiltshire which occurred on the 30th December, 1859. 8vo. 45 pp. [Read at meeting of British Meteorological Society, 21st March, 1860.] *Oxford*, 1860

—— On Electricity as the Expansive Force of Steam. Letter to the Secretary of the Smithsonian Institution, Washington, August 19, 1873. 8vo. 7 pp. *Oxford*, 1873

—— Meteorological Phenomena. Why the Rain Falls. Slip. [Reprinted from the *Oxford Times*, July 19, 1879.] *Oxford*, 1879

Rowell [G. A.] On the Storm in the Isle of Wight, September 28, 1876, and on the Cause of Storms. 8vo. 44 pp. *Oxford*, 1876

—— Meteorological Phenomena. Recent Sunsets and Sun Risings. Slip. [Reprinted from the *Oxford Times*, January 5, 1884.] *Oxford*, 1884

Royal Observatory, Greenwich. Results of the Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1883, under the direction of W. H. M. Christie, M.A., F.R.S. La. 4to. lii + lxxxi pp. *London*, 1885

—— Rates of Chronometers on Trial for Purchase by the Board of Admiralty at the Royal Observatory, Greenwich, 1885. 4to. 7 pp. *London*, 1885

—— Report to the Board of Visitors of the Royal Observatory, Greenwich. [Read at the Annual Visitation of the Royal Observatory, June 6, 1885.] *London*, 1885

Royal United Service Institution. Journal. Vol. XXIX., No. 139, pp. 823 to 1002. *London*, 1885

* **Schlagintweit** [Hermann]. Report on the Progress of the Magnetic Survey of India, and the Researches connected with it in Sikkim, Khosia Hills, and Assam, from April to December, 1855. La. 8vo. 32 pp. *Madras*, 1856

Sieur [M.] Étude sur la Téléphonie. 8vo. 137 pp. *Paris*, 1885
[Presented by J. Aylmer, Esq., Local Hon. Sec., Paris.]

Simon [Maurice]. Aperçu Pratique sur l'Éclairage Électrique Renseignements Commerciaux. Explication des principaux termes. 8vo. 132 pp. *Paris*, 1885

Slingo [W.] Electrical Education. 4to. 2 pp. [*Knowledge*, June 13, 1884.] *London*, 1884

Smith [F. J.], B.A. On some New Forms of Work-measuring Machines as applied to Dynamos and Electro-motors. 8vo. 32 pp. *London*, 1885

Smith [M. Holroyd]. [*Vide* Manchester Association of Employers, &c.]

Société Francaise de Physique. Collection de Mémoires relatifs à la Physique. Tome II. Mémoires sur l'Électrodynamique. Première Partie. 8vo. 412 pp. *Paris*, 1885

Society of Arts. Index to Vols. I. to X. of *Journal of Society of Arts.* 8vo. 57 pp. *London*, 1863

—— Index to Vols. XI. to XX. of *Journal of Society of Arts.* 8vo. 91 pp. *London*, 1873

Society of Engineers. Transactions for 1884. 8vo. 208 pp. *London*, 1885

* **Stephen** [Vincent]. Wrinkles in Electric Lighting. 8vo. 45 pp. *London*, 1885

Thomson [Sir Wm.] [*Vide* Collet.]

Thue. [*Vide* Clauson-Thue, W.]

Todd [Charles]. Telephone Regulations, General Post Office, Adelaide. Sept., 1883. Fo. 1 p. *Adelaide*, 1883

Tommasi [Dr. Donato]. Éclairage des Trains de Chemin de Fer par l'Électricité combinée avec le Gaz. 8vo. 23 pp. [Reprinted from the *Moniteur Industriel.*] *Paris*, 1884

—— List of the Papers, &c., of. 4to. 2 pp. 1885

—— Sur la Loi des Constantes Thermiques de Substitution. 8vo. 3 pp. *Paris*, 1885 (?)

Tommasi [Dr. D.] **et Radiguet.** Pile à Électrodes de Charbon. 18mo. 4 pp. Paris, 1885 (?)

Treuenfeld [R. von Fischer]. Militär-Telegraphie. La. 8vo. 16 pp. [Separat-Abdruck aus der *Zeitschrift für Elektrotechnik*, 1885.] *Wien*, 1885

United States of America. Annual Report of the Commissioner of Patents for the year 1884. 4to. 645 pp. *Washington*, 1885

—— Ordnance Notes. Vol. XI. [Nos. 320 to 333, inclusive.] La. 8vo. *Washington, D.C.*, 1885

—— Annual Report of Chief of Ordnance [Brigadier-General S. V. Benét] to the Secretary for War for the Fiscal Year ended June 30, 1885. 8vo. 27 pp. *Washington*, 1885

University College. Calendar. Session MDCCCLXXXV.—LXXXVI. 8vo. 368 + lxxxvi pp. *London*, 1885

War Office [London]. Army Telegraphs. [Instructions for working the Army Telegraphs in Egypt during the Egyptian Campaign.] Fo. 8 pp. [Received 21st May, 1885.]

—— Military Telegraphs. [Working of the Military Telegraphs in Egypt during the Egyptian Campaign.] Sm. 8vo. 38 pp. [Received 21st May, 1885.]

[Presented by Major-General C. E. Webber, R.E., C.B., Past-President.]

—— [*Vide* McCollough.]

Watkin [Major H. S. S.], R.A. Modern Slide Rules. 8vo. 5 pp. [Reprinted from *Proceedings, Royal Artillery Institution*, No. 13, Vol. XIII.] *Woolwich*, 1885

Webber [Major-General C. E.], R.E., C.B. [*Vide* War Office.]

Weiller [Lazare]. [*Vide* Grief, J. B.]

* **Weissenbruch** [L.] Les Applications de l'Électricité aux Chemins de Fer. Fo. 61 pp. *Paris*, 1885

West [John], C.E. The Manufacture and Distribution of Gas, with special reference to the Modern Apparatus and Machinery now employed at Gas Works. 8vo. 28 pp. [Manchester Association of Employers, &c. Nov. 22, 1884.] *Manchester*, 1884

Wharton [C. J.] [*Vide* Hospitalier.]

* **Williams** [W.] Manual of Telegraphy. 8vo. 327 pp. *London*, 1885

Zetzsche [Prof. Dr. K. E.] Handbuch der Elektrischen Telegraphie. Dritter Band—Die Elektrische Telegraphie im Engeren Sinne—Erste Hälfte. Vierte Lieferung. 8vo. 127 pp. [pp. 481 to 608]. *Berlin*, 1885

ORIGINAL COMMUNICATION.

ON THE DETERMINATION OF CHEMICAL AFFINITY IN TERMS OF E.M.F.

By C. R. Alder Wright, D.Sc., F.R.S.

During the last eight years the author has carried out (partly by himself, partly in conjunction with Dr. Rennie and with Mr. C. Thompson) a lengthy series of observations on certain points more or less intimately connected with this problem.* Whilst numerous matters of interest have been examined, and a large number of numerical data have been fixed as accurately as practicable, the main object remains still unattained, inasmuch as all the results arrived at so far only lead to the conclusions that electrical determinations alone are unlikely to afford the means of readily obtaining exact measurements of the amounts of force and energy involved in the occurrence of chemical changes, at least so far as the more deep-seated phases of these reactions are concerned.

The fundamental idea involved in the conception of the possibility of such measurements being made was put forth in 1851 by Sir William Thomson (*Phil. Mag.*, vol. ii., p. 429), in a remarkable paper on the "Mechanical Theory of Electrolysis." From Faraday's experiments it results that when electrolysis occurs the weight of substance decomposed varies—firstly, as the chemical equivalent of the substance; and secondly, as the quantity of electricity passing. Or $w \propto a\,q$, where w is the weight of substance decomposed, a the equivalent, and q the quantity of electricity. Hence $w = a\,q \times \mathrm{F}$, where F is a

* These results have been mostly embodied in a series of nine memoirs, read from time to time before the Physical Society of London, and published in its proceedings; and in the *Phil. Mag.*

constant numerical value which may be conveniently designated the Faraday coefficient (as J is the Joule coefficient for conversion of units of heat into mechanical units). If f represent the work done by the force of chemical affinity in synthesising a unit of weight of the substance decomposed from the products of decomposition, the work done may also be written $w f = a q \mathrm{F} f$. Now suppose the passage of the current to effect no other work than chemical decomposition, or, what amounts to the same thing, let the total work done be corrected by subtracting the quantity done as heat evolution, &c., so that the balance represents chemical decomposition; let the lowering of potential (E.M.F.) occurring between the extremities of the mass through which the current passes be (after this correction) e; then the work done as chemical change is $e q$ and also $= a q F f$, whence $A f = e \times \frac{1}{F}$. That is, the value of the chemical affinity per gramme equivalent of compound is measured by a value in E.M.F. multiplied by a constant.

Of the heat evolved in the passage of current through an electrolyte a certain portion is due to the resistance proper of the medium, being in accordance with Joule's law $\mathrm{C}^2 \mathrm{R}\, t$, where C is the current, R the resistance, and t the time of passage. In most cases the heat actually evolved differs from this quantity, usually (but not invariably) exceeding it. When actual decomposition unmodified by secondary changes involving alteration of the electrodes is effected (*e.g.*, acidulated water with platinum electrodes), the amount of heat absorbed in the act of breaking up the electrolyte into the final free products of decomposition (*e.g.*, free oxygen and hydrogen at the ordinary temperature and pressure) can be calculated by observing the mean potential difference subsisting during the experiment, and subtracting therefrom the E.M.F. corresponding with the heat developed (ascertained by the calorimeter). Conversely, if the heat set free during synthesis of the electrolyte from the final products be known previously, such observations afford a means of deducing on the one hand a valuation of J, or on the other of the E.M.F. standard employed (which in practice amounts to a valuation of

the resistance unit). Some of the earliest of these researches made in conjunction with Dr. Rennie led to the following result—viz., that whilst the most trustworthy calorimetric experiments on record give 34,100 grammes of water raised from 0 to 1° C. as the heat evolution during synthesis of 8·98 grammes of water from 1 gramme of hydrogen and 7·98 grammes of oxygen at ordinary temperature and pressure, and whilst this value corresponds with $1{\cdot}5038 \times 10^8$ C.G.S. units, taking the most probable values (at that time) for F and J, the actually observed E.M.F. (deduced from the heat evolution as measured by the calorimeter subtracted from the mean potential difference between the electrodes) was $1{\cdot}5003 \times 10^8$, with a probable error of $\cdot 0048 \times 10^8$. When corrected in accordance with more recent researches—more especially those of Lord Rayleigh—these values are found to be equally consistent with the present more accurately known values for the B.A. resistance unit and J, and, taken in conjunction with Joule's most trustworthy results afford an additional indication that the B.A. unit of resistance is incorrect by upwards of 1 per cent.

The heat evolution due to resistance proper, $C^2 R t$, may be written $e^1 q$, where q is the quantity of electricity passing as before, and e^1 a value in E.M.F. If E represent the potential difference between the electrodes, $E - e^1$ will represent the E.M.F. which corresponds with the work done in effecting the decomposition of the electrolyte into "nascent" products, often spoken of as *counter electro-motive force*. When the actual heat evolution differs from $e^1 q$, it is because secondary actions take place, either through the action of the "nascent" products upon the electrodes, or through their own spontaneous alteration forming the permanent products (thus it may be supposed that free atoms, say of hydrogen and oxygen, are primarily evolved, and these "nascent" forms coalesce subsequently into molecules). A large number of observations have been made on the actual values of the quantity $E - e^1$ with various electrolytes, electrodes, and current densities; these experiments as a whole lead to the conclusion that whenever secondary actions take place, whether by spontaneous rearrangement of "nascent" products or by the

action (physical or chemical) of these upon the electrodes, a portion, varying with circumstances, of the energy gained by these secondary causes tends to aid the primary current, thus bringing about a less lowering of potential between the electrodes than would otherwise be requisite. In certain cases this *adjuvant* action due to secondary causes is of such magnitude as to more than counterbalance the fall in potential due to primary causes, so that on the whole, instead of a lowering, a raising of potential ensues as the current flows. Cells where this occurs do not require a current *ab externo* at all to create potential difference; they constitute voltaic elements or electro-motors, but in all respects they are subject to the same influences as decomposing cells in which actual fall of potential is brought about, excepting that the direction in which the influence works is opposite. Thus, *cæteris paribus*, the value of the counter E.M.F. $E - e^1$ in a decomposing cell invariably *increases* with the current density. With a voltaic element in which $E - e^1$ is opposite in sign (negative counter electro-motive force), the numerical value of this quantity (*i.e.*, the E.M.F. of the cell viewed as a generator of electricity) *decreases* with increasing current density. In both cases the variation is due to the same cause, viz., that the amount of *adjuvant* energy due to secondary causes decreases and the amount of *nonadjuvant* energy increases with increasing current density.

In connection with this subject the experiments carried out naturally fall into two classes, viz., those made with decomposing cells and those with electro-motors; and in each case phenomena of interest have been examined, varying according as currents of minute or of considerable density were used. Firstly, as regards decomposing cells, a number of observations have been made on points connected with the physical adherence of products of decomposition to the electrodes; this physical adherence causes the cell to possess the property of giving a current spontaneously in the reverse direction for a longer or shorter time after the primary current is broken, the films of decomposition products adhering to the electrodes giving to the whole the character of a voltaic electro-motor. Whilst examining this subject, more

especially with reference to the decomposition of acidulated water with platinum electrodes, an amount of evidence was gained tending to prove that the sometimes alleged possibility of passage of electricity through an electrolyte without producing decomposition is actually non-existent, electrolysis always occurring but the products not becoming visible—firstly, through their adherence to the electrodes, and, secondly, through their gradual removal therefrom by solution and diffusion; in other words, Faraday's law of proportionality between quantity of electricity passing and amount of decomposition holds even with the feeblest currents. In the course of these experiments it was found that electrolysis of acidulated water may take place with potential differences very far below 1·5 volt, the E.M.F. corresponding with the heat of formation of liquid water from gaseous hydrogen and oxygen; in fact, an E.M.F. of less than 0·1 volt, and even a barely measurable E.M.F., will suffice; which appears to indicate that the *adjuvant* energy due to the physical attractions between the evolved products and the electrode surfaces is very large, approximating to 1·5 volt under suitable conditions; or, in other words, the heat evolution during the condensation of hydrogen on the one hand and oxygen on the other to attracted or occluded films or auras of adherent gas, amounts in the aggregate when at its maximum to something not far short of, if not exceeding, 34,100 gramme degrees per gramme-equivalent of the two elements severally and jointly.

With currents of sufficient magnitude to produce visible evolution of gas the value of $E - e^1$ for water invariably exceeds 1·5 volt. A large number of experiments have been made with the view of obtaining accurate co-ordinate values for curves correlating density of current and counter E.M.F. for different sorts of electrodes (platinum, gold, carbon, &c.). These curves, if capable of mathematical expression by exponential equations, or otherwise suitably, should lead to limiting values for $E - e^1$ when the density becomes infinite. Various approximations in this direction have been obtained with different substances; but exact figures are very difficult to deduce, and require much laborious work. For water the limiting value has been found to

be at least upwards of 4 volts; *i.e.*, the heat development during the re-arrangement of "nascent" hydrogen and oxygen into the ordinary gaseous forms of these bodies is in the aggregate not far short of double that evolved when the free gases unite to form liquid water; and similarly with other electrolytes. The details of these experiments are as yet mostly unpublished.

With electrodes of materials capable of being attacked by the products of electrolysis similar curves are obtainable; the amount of *nonadjuvant* energy due to this secondary action on the electrode again increasing with the current density, but being also dependent on the temperature and the degree of concentration of the solution electrolysed. Further, under precisely similar conditions as to density of current, temperature, and fluid electrolysed, the amount of *nonadjuvant* energy is influenced by the nature of the electrode material, being usually less the more readily oxidisable the metal constituting the positive electrode. Thus of the four metals, zinc, cadmium, copper, and silver (the oxidisation heats of which lie in the order of the names), the first gives a lesser amount of *nonadjuvant* energy than the second, when a sulphate is electrolysed so as to form zinc sulphate at the positive electrode on the one hand and cadmium sulphate on the other; all other conditions being constant, similarly the second gives less than the third, and the third than the fourth.

As regards electro-motors, much labour has been expended in studying the behaviour of certain forms of cell capable of being usefully employed as practical standards of E.M.F. Of these the two most convenient are found to be the "Clark" cell (mercury—mercurous and zinc sulphates—zinc) and the Daniell when set up with solutions of pure zinc and copper sulphates of equal molecular strength (almost exactly equal in sp. gr.), zinc plates of pure cast metal freshly amalgamated with pure mercury, and copper plates recently electro-coated with pure pink copper. The effects on the E.M.F. of these and many other analogous cells produced by variations in the nature of the active plate surfaces and the strength of the metallic salt solutions, have been studied and expressed by curve values, some of which lead to interesting results: thus, whilst increasing the strength of the

zinc sulphate solution in a Daniell cell diminishes the E.M.F., and increasing that of the copper sulphate increases it* (and similarly, *mutatis mutandis*, with all similarly constructed cells), the rate of increase or decrease also depends upon the nature of the surface of the metallic plate, differing according as this is of bright cast or rolled metal, electro-coated, or amalgamated, &c. All these results, as well as the dependence of the proportion of *adjuvant* energy from secondary reactions on current density, unmistakably point to the conclusion that the prime seat of potential alteration lies at the junction of the different surfaces of electrodes and fluids, and that voltaic action is really closely allied to thermo-electric action.

The alterations in the E.M.F. of various forms of electro-motors with increasing current density have been studied and expressed by curves; very considerable diminutions are thus brought about with moderate rates of current flow. Thus with current densities not exceeding a few thousandths of an ampère per square centimètre, diminutions of some decivolts are in some cases brought about. Other things being equal, the amount of diminution for a given increment in current density is found to be greater the less oxidisable the metal unattacked by the chemical action taking place. Thus, when zinc is successively opposed to cadmium, copper, and silver in sulphate cells, the curve representing the variation in the E.M.F. with current density in the first case

* When the plate corresponding with the copper of a Daniell cell is surrounded by a mixed solution of two salts (*e.g.*, zinc and copper sulphates), some remarkable variations in the E.M.F. of the combination are sometimes brought about by varying the proportion of the two salts and their absolute quantities relatively to the solvent water. In certain cases increasing the amount of the second salt (corresponding with the zinc sulphate in the case supposed) continually increases the E.M.F.; in other instances it continually decreases the E.M.F.; within certain limits of range it first causes a decrement, and subsequently an increment, so that a minimum value is observed when the varying salt reaches a particular amount, whilst two different proportions of that salt can be found either of which will yield the same E.M.F. somewhat above the minimum. Expressed as curve values, for certain cases the curve continually ascends above the base line, and in others continually falls below it; and in some instances first falls below the base line to the minimum and then reascends.

underlies that similarly obtainable in the second, which, again, underlies that obtainable in the third case. The precise amount of diminution in E.M.F. (*i.e.*, the proportion of *nonadjuvant* energy) is found to be variable within certain limits with the physical condition of the surface of the plates (whether bright, electro-deposited, amalgamated, &c.), and with the concentration of the solution surrounding it.

All these experiments tend to show that the exact amount of (positive or negative) lowering of potential actually brought about by the passage of a current, although primarily due to the balance of chemical affinities involved, is nevertheless secondarily influenced by so many circumstances as to render it extremely difficult, if not impracticable, to obtain any accurate estimations of the values of these affinities by the combination of observations under circumstances varying in known ways, especially when currents of any considerable magnitude are employed. On the other hand, some remarkable results were obtained on studying the maximum E.M.F's generated by a large number of voltaic combinations when producing currents of insufficient density to cause any noticeable fall in E.M.F. through *nonadjuvancy*, and comparing these values with those corresponding with the heat development due to the net chemical change taking place. In order to express the E.M.F. values corresponding with any given combination of metals, &c, it is convenient to assign numerical values, or *voltaic constants*, to each metal when immersed in a solution of one of its salts of a given strength, these constants representing the E.M.F. set up when each plate and fluid are connected Daniell-cell fashion with amalgamated zinc immersed in the corresponding zinc salt solution of equal molecular strength. A large number of carefully conducted experiments show that the effect of a given alteration in the molecular strength of either solution in such a combination is independent of the nature and strength of the opposed metal and salt solution, and that Volta's law of summation rigorously holds; hence the value of the E.M.F. of any given voltaic combination is obtainable by simply taking the algebraic difference between the voltaic constants pertaining to the particular metals, salts, an

solution-strengths employed. In the same kind of way a series of *thermo-voltaic constants* is calculable, such that the E.M.F. of any given combination (expressible by the formula $C_1 - C_2$, where C_1 and C_2 are the appropriate voltaic constants) is given by the formula $E_H + K_1 - K_2$, where E_H is the E.M.F. corresponding with the heat evolution due to the net chemical change, and K_1 and K_2 the thermo-voltaic constants corresponding respectively with C_1 and C_2; that is, the E.M.F. actually generated may be regarded as due to two superposed causes—viz., the net heat development through chemical change, and a thermo-voltaic action akin to that taking place in an ordinary thermo-couple, this latter being variable (within certain limits) with every circumstance modifying the relations to one another of the contiguous surfaces of metal plate and surrounding fluid. On comparing together the values of K_1 and K_2 for various metals, &c., and those of $E_H + K_1 - K_2$ for various combinations, some noteworthy results are deducible. Firstly, metals generally may be divided into the two classes—those which have a more or less considerable positive thermo-voltaic value, and those which have a negative one. In general the sign is the same for a given metal in contact with all its salts; but in some few cases where the thermo-voltaic constant has never any large value (*e.g.*, copper and cadmium), the sign is sometimes +, sometimes −; thus silver and lead invariably give more or less considerable − values; whilst iron, mercury, magnesium, and aluminium always give + ones. Nextly, cells may be divided into two classes, viz., those where $K_1 - K_2$ has a + value, and those where it has a − one. The former class of cell is remarkable in that the E.M.F. actually generated necessarily exceeds that due to the net chemical change; so that when a current circulates the extra energy capable of being obtained in an external circuit of high resistance, as compared with the cell itself, over and above that due to the chemical action, is derived at the expense of sensible heat, *i.e.*, the cell is *cooled* by the action. In the second class of cell the reverse is the case so long as E_H is numerically greater than $K_1 - K_2$, the work capable of being done externally being always less than that due to the

chemical change; when, however, the numerical value of $K_1 - K_2$ exceeds that of E_H, a most remarkable result ensues—the current flows in the direction *opposite* to that predicable from the relative heats of formation of the two metallic salt solutions used; instead of the plate in the solution of lower heat-formation (the copper in a Daniell's cell) acquiring the higher potential, it acquires the lower one; in consequence the action of the cell is accompanied by a twofold heat absorption: firstly, the chemical action itself is accompanied by heat absorption; and, secondly, any work done by the current outside the cell must be due to conversion of sensible heat into current energy.

A class of cell has also been examined in which the net chemical action is nil, the cell consisting of two identically-surfaced metallic plates immersed in solutions of a salt of the metal of different strengths. In such a combination the only source of energy is the possible heat evolution due to admixture of the two differently saline fluids. In many instances, however, the E.M.F. actually set up far exceeds that due to this cause, indicating conversion of sensible heat into current energy. In the case of zinc, cadmium, and copper sulphates, comparisons were made between the E.M.F.'s generated with solutions of varying strengths on inter-diffusion, the amounts of heat developed on intermixture, and the decrements in volume ensuing thereon; with the result of indicating that whilst contraction on intermixture and development of E.M.F. run tolerably concurrently together, the development of heat on intermixture does not vary at all *pari passu* with either. The E.M.F. set up, however, not only varies with the particular strength-solution difference, but is also a function of the character of the surfaces of the plates; thus amalgamated zinc plates in solutions of zinc sulphate give larger E.M.F.'s for given strength-differences than plates electro-coated with zinc, and these larger values than bright zinc plates; and similarly with other metals.

On the whole, the general result deducible from the examination of a very large number of different voltaic combinations is that a concordance to within $\pm 0{\cdot}1$ volt between the E.M.F. *calculable* from the net chemical change and that actually set up

is comparatively seldom observed, the two values most frequently standing in no close relationship to one another even when only infinitesimal currents flow; whilst far greater sources of divergence between the values are afforded if currents are developed of any moderate magnitude; so that the actual development of E.M.F. in most voltaic combinations is in no way calculable from the resultant of the various chemical affinities involved in their action.

ABSTRACTS.

(We are indebted to the courtesy of the Institution of Civil Engineers for allowing the first seven of the following Abstracts to be reprinted from their Proceedings.)

A. CROVA—PHOTOMETRY OF INTENSE FOCI OF LIGHT.

(*Comptes Rendus, Vol.* 99, 1884, *pp.* 1067-69.)

A complete determination of the photometric value of an intense focus of light, electric or solar, requires the practical realisation of the following conditions:—(1.) Comparison of two lights of different tints. (2.) Evaluation of the tint by means of a numerical factor. (3.) Determination of the photometric ratio of a very intense source in function of a relatively feeble standard.

The first may be resolved by one of two methods previously described* by the author, which admit of reducing the comparison of the total lights to that of the relative intensity of a simple light, conveniently chosen, taken in the two sources; the more simple consists in the use of a solution of perchloride of iron and chloride of nickel, through which a photometric screen is observed.† With a thickness of about 7 millimètres this solution stops all but radiations comprised between wave-lengths of 630μ and 534μ, with a maximum towards 580. The simple radiations transmitted with maximum of intensity chiefly include all those giving the same ratio as that of total illuminations with the standard Carcel with relation to the luminous sources, the tint of which varies between the reddest, that of the Carcel, and the whitest, that of solar light. In the second case the author has shown‡ how the use of the spectro-photometer admits of expressing the temperature of a luminous source in arbitrary optical degrees. In practice, the tint may be easily represented by means of two successive photometric determinations: one, obtained by observation of a Foucault photometric screen through the 580 solution, gives the ratio of the intensities; the other, made by placing before the eye a red (protoxide of copper) glass which allows of the passage of radiations included between 726μ

* *Comptes Rendus*, vol. 93, p. 519; and vol. 96, p. 1271.

† The most suitable solution is the following:—

	Grains.
Perchloride of iron, anhydrous and sublimed	22·321
Chloride of nickel, crystallised	27·191

dissolved in distilled water to a total volume of 100 cubic centimètres at 15°. To avoid all possibility of reduction of the perchloride of iron, the solution, saturated with chlorine, is enclosed in a cell formed of a ring of ground glass, against which are pressed two sheets of glass by means of a frame of blackened brass provided with screw pressure.

‡ *Comptes Rendus*, vol. 90, p. 252; and vol. 92, p. 70.

and 752μ, with a maximum at 650μ, gives a ratio as inferior to the preceding as the tint of the light compared to the Carcel is whiter. The quotient of the first determination by the second admits of characterising the tint: it is greater as the light is whiter; it is equal to unity for sources of the same tint as the Carcel. For a glow lamp it varied, during the author's experiments, from 1·05 to 1·23, according to the intensity of the current, whilst the luminous intensity was raised from 1·1 to 3·2 Carcels. For arc lamps the coefficient representing the tint is greater; it attains the value 1·5 to 1·7, with a Serrin lamp carrying Carré carbons of 12 millimètres in diameter, fed from a "workshop" Gramme dynamo, giving 230 to 320 Carcels intensity, and expending between the points an electric work of 150 to 166 kilogrammètres per second. Greater intensities will probably be represented by higher numbers.

With sunlight the tints are represented by numbers increasing with the height of the sun, and greater than the preceding.

The author proposes to follow the working of incandescent lamps with relation to the electric energy expended, and to stop the degree of incandescence and of whiteness of the light at a higher limit expressed by a numerical coefficient that determines the best conditions of intensity and of whiteness compatible with sufficiently long life of the lamp.

A. CROVA and P. GARBE—DETERMINATION AND REGISTRATION OF THE CHARGE OF ACCUMULATORS.

(*Comptes Rendus*, *Vol.* 100, 1885, *pp.* 1340-43.)

Admitting that the chemical reactions produced on the two plates of an accumulator are, during charging, the transformation of the layer of sulphate of lead on the positive plate into binoxide of lead, and that on the negative plate into metallic lead, the charge corresponding to the decomposition of 1 equivalent of sulphate of lead on each plate will transform—

1. At the positive pole, PbO, SO^3 into PbO^2, liberating 1 equivalent of $SO^3 HO$.
2. At the negative pole, PbO, SO^3 into Pb, liberating 1 equivalent of $SO^3 HO$.

The electro-chemical equivalent of lead being 1·0867 milligramme, and that of the acid 0·51445 milligramme, each coulomb stored will act on 1·0867 milligramme of lead on each plate, setting free 1·0289 milligramme of acid.

The authors have experimented with some Faure cells of the 40 ampère-hour type, the active matter of which weighs 3 kilogrammes, and charged with 1 litre of water containing $\frac{1}{10}$ the volume of acid. The liquid then contains 184 grammes of sulphuric acid, which to enter entirely into combination would require 388 grammes of lead. Allowing that the accumulators receive 40 ampère-hours or 144,000 coulombs, this charge corresponds to the reduction of 155·8 grammes of lead on one of the plates, and the conversion into binoxide of an equal weight of sulphate from the other. The quantity of sulphuric acid set free

will then be 149·25 grammes, or 3·73 grammes per ampère-hour. For a charge of 40 ampère-hours the decrease of weight of the plates, and the increase of weight of the liquid, will be respectively about 150 grammes. This motor force, proportional to the charge, is fully sufficient to indicate its variations, and even to register them.

In a trial the variation of weight was 138 grammes. The concurrence is deemed by the authors sufficiently close to base upon a system of weighting the liquid or the plates a method of registration of the charge.

TSCHELTZOW—THE THERMO-CHEMICAL STUDY OF ACCUMULATORS.

(*Comptes Rendus, Vol.* 100, 1885, *pp.* 1458-60.)

To fulfil calculations of the electro-motive force of accumulators by Joule and Thomson's law, $E = 0·0436 \Sigma Q$ (where E is electro-motive force in volts, and ΣQ the heat, relative to the equivalents, in calories) it is requisite to determine the heat of formation of peroxide of lead from oxide of lead and free oxygen. This has been done by two different processes—(1) the action of nitrate of protoxide of mercury dissolved in dilute nitric acid on the peroxide of lead; (2) the action of anhydrous sulphurous acid on the peroxide. As means of four experiments (1) gave 31·85 calories, and (2) 82·62 calories; whence is calculable 12·14 calories (towards 17°) as a mean of both processes.

The heat of formation of peroxide of lead permits of the examination of the chemical reactions occurring on the two plates of an accumulator. There are four hypotheses examined. If PbO_2 were reduced into Pb by the hydrogen at the negative pole, the reaction would disengage 37·20 calories, corresponding to 0·81 volt. If PbO_2 were transformed into $PbSO_4$, the disengagement of heat would be 81·50 calories, corresponding to 1·77 volt. If the negative lead were transformed into sulphate, and PbO_2 into Pb, there would be disengaged 44·30 calories, corresponding to 0·96 volt. If there be sulphatation of the two electrodes, the heat disengaged will be 81·60 calories, corresponding to 1·93 volt, which the author considers the true fundamental reaction.

BLAVIER—INFLUENCE OF STORMS ON SUBTERRANEAN TELEGRAPH LINES.

(*Comptes Rendus, Vol.* 100, 1885, *pp.* 1534-35.)

When the construction of the long subterranean lines in France was commenced some years ago, it was thought that their conductors would be quite free from the effect of storms. Accidents are much rarer than with aerial lines, but that they do occur was evidenced by the violent storm on the 9th March at the middle of the underground line connecting Belfort with Besançon, when sparks appeared at the two extreme stations. This phenomenon, apparently contrary to the theory of static electricity, the author

thinks may be explained either as an effect of electro-dynamic induction or as an effect of electro-static induction. If the cable is buried only to a small depth in a badly-conducting earth, the sheathing takes, under the influence of storm clouds whilst the internal wire remains neutral, a more or less considerable electric charge. At the discharge of the clouds this charge becomes free and escapes into the earth, following the sheathing in two opposite directions. This will develop in the interior conductor two induced currents of contrary directions, the difference of which will act on the apparatus at the end stations. Or the conductor may charge itself from the earth-plates and lightning-guards under the influence of this sheathing-charge.

GIRAUD—GELLERAT'S ELECTRIC ROAD-ROLLER.

(*Comptes Rendus de la Société des Ingénieurs Civils, Paris*, 1885, p. 320.)

Towards the end of 1883, Mr. E. Gellerat made a successful application of dynamo-electric power to drive a road-roller. He adapted the electric machinery to the framing and roller of an existing steam roller, from which the boiler and machinery was removed, leaving a platform about 18 feet long and 6½ feet wide, carried on cast-iron rollers 4 feet and 4½ feet in diameter, weighing in all from 10 tons to 11 tons. On this platform, in three stages, 104 Faure accumulators, weighing about 130 lbs. each, were deposited, making a load of about 6 tons. The dynamos and the machinery weigh about 1 ton. Together, the gross weight of the road-roller amounts to 18 tons. Steam cylinders for the same weight work to from 10 H.P. to 15 H.P., and to much more on an emergency; and for this power two Siemens dynamos of the type D^2 on one shaft are provided, capable of exerting 12 H.P., and more than that by increasing the velocity. A small dynamo, type D^1, of 1¼ H.P., is supplied for the steering power.

The motion of the dynamos is reduced and transmitted by suitable gearing to the intermediate shaft, whence it is transmitted in the usual manner to the rollers. The accumulators have each a power of about 2 volts, making together about 200 volts. Seventeen accumulators were reserved for the steering when it was necessary. The remainder were employed, more or less, for locomotion; but on firm ground 50 accumulators sufficed for this purpose, with an intensity of current of from 30 to 40 ampères, representing from 4 H.P. to 5 H.P., although the speed attained did not exceed 1¼ mile per hour.

The roller was taken over some newly-laid macadam of a thickness of from 8 inches to 10 inches, bedded on a clay substructure, over a newly-constructed sewer, on an incline of from 1 in 50 to 1 in 33. All the accumulators were brought into action; the rolling commenced at a speed of from 2 to 2½ miles per hour, and was continued for three hours with as much facility as if the machine had been worked by steam. The expenditure of electricity was proportional to the resistance of the ground. The intensity of the current averaged 35 ampères, and attained at one point to 75 ampères, which, for 104

accumulators, corresponded to the work of 20 H.P. At the end of the trial, which was satisfactory, the accumulators had worked for four hours, and were not half discharged.

J. WYBAUW—A PHOTOMETER FOR ELECTRIC LIGHT.

(*Bulletin de la Société Belge d'Électriciens, Vol.* 2, *January*, 1885, *pp.* 5-12.)

The author proposes to find a means of overcoming or diminishing the difficulty met with in ordinary photometers for the direct comparison of the electric light with the flame of a Carcel lamp or gas-burner. The difference of colour in two such lights is so considerable as to render the accuracy of such an operation uncertain. Using Foucault's photometer, the comparison is nearly impossible; with the Bunsen photometer the difficulty is somewhat less, because by experience one can tell pretty exactly the moment when the edge of the little translucent spot becomes vague and disappears more or less. In any case the uncertainty is great, and the results vary with the observer, and even between two sets of observations by the same person.

Let unit of illumination be that furnished at unit distance by a source equal to the light of unit intensity. The curve representing the illuminations given by this source at different distances from the origin would be $y = \frac{1}{x^2}$, and for a source of intensity I, it would be $y = \frac{I}{x^2}$, so that by making $x = 1$ in the equation to the curve of illumination of a source, there is obtained an ordinate $y = I$, which contains as many units of illumination as the source does of units of intensity. To compare two sources, I and I′, it suffices to consider the differences between the ordinates of the two curves of illumination at the same horizontal distances from the origin. This consideration led the author to construct his photometer, which consists of two mirrors, m and n, some distance apart, and inclined at 45° to the rays of light coming from the source O. These rays are reflected on two screens p and q, perfectly alike, and formed of white paper. The whole is fixed in a rectangular box 50 centimètres long by 40 wide, blackened inside and provided with screens, to prevent useless diffusion and reflection of light. The observer ascertains when the screen p is more lighted than q. The difference of the two illuminations can be measured with another known light, C, by which the illumination of the screen q is increased until it is equal to p; or inversely, if C remain fixed, the source O can be moved backwards and forwards until equality is established. If O is an electric light, the screen q is illuminated by rays of electric light, but at the same time by a small portion of yellow light from the Carcel lamp C, to make the tints on screens p and q alike. The intensity I of the source O is given with great accuracy by a general formula, which may be simplified to the form

$$I = y\,\frac{z^2}{x^2},$$

where x denotes the distance O m p, and z the distance C q, to be measured for

each experiment. On the cover of each photometer there is a curve which gives the value of y for any value of x whatever.

If x = constant, the intensities I and I′ of the two sources by the general formula are $M\frac{C}{z^2}$ and $M\frac{C}{z'^2}$, where $\frac{C}{z^2}$ and $\frac{C}{z'^2}$ express the differences of the illuminations in p and q. This gives an easy means of comparing the intensities of different sources by a lamp of type C. One advantage of this photometer is that it is possible to measure an electric light in a confined space. Calculations are easiest with C constant, but this condition is frequently difficult to realise practically. The author investigates the various formulæ, and works out several numerical examples.

Transparent screens p and q could be employed as in Foucault's photometer; and another less practical arrangement is given, resembling Bunsen's photometer.

The method employed by the author to realise the greatest equality of colour on the screens does not lead to any likelihood of error due to the diminution in the lengths of the distances compared. In this, as in ordinary photometers, it is the distance z of the standard light from the screen which determines the result of the operation. Numerical examples are given to show that errors of observation do not affect the results more seriously in this photometer than in that of Foucault or Bunsen, whilst by the plan of illuminating one screen with a light three-quarters to four-fifths that of the other screen, the distances are very much diminished, and, the colours being brought to the same tint, the errors are reduced to a minimum. The use of coloured glasses or solutions interposed between the eye and the images on the screens renders the tints really equal; but this expedient is useless here, and can only destroy the correctness and delicacy of the observations, by rendering the little differences less sensible.

J. TROWBRIDGE—A STANDARD OF LIGHT.

(*The American Journal of Science, Vol.* 30, 1885, *p.* 128.)

The author thinks the adoption by the Paris Conference of 1881–84 of the light emitted by a surface of platinum at the point of solidification as a standard of light did not assort well with discussions which reaffirmed the C.G.S. system of units; as although it might be a fixed point in nature, it had not been shown how this point could be connected with the units of heat and work, and electrical energy. As there ought to be such a connection, the suggestion of Schwendler that the light emitted by a strip of platinum rendered incandescent by a known electrical current should be adopted as a standard, seems, in the author's opinion, to merit more attention than it has received. The first to propose such a standard was Dr. John W. Draper, of New York, in 1847 ("Scientific Memoirs," p. 45).

The author found, upon trial, that there were very great difficulties in the

use of fine platinum wire as a standard, on account of the necessity of measuring light and potential of current at the same instant, the wire fusing before the measurements could be completed. He succeeded, however, with a strip of platinum foil placed in the shunt circuit of a small dynamo machine, the light from which could be maintained very constant with the proper speed and suitable adjustment of resistances. The light emitted by the platinum was measured by means of a Ritchie photometer, and the electric current by a tangent galvanometer and Thomson quadrant electrometer The range of the indications of the electrical instruments was comparatively small, while the light varied enormously, the difficulty of the method being that of measuring a strong current with accuracy.

After failing to measure changes in the heat radiated by glow lamp by means of a thermo-junction in the centre of the carbon loop, the author substituted a loop of extremely fine platinum wire to form one branch of a Wheatstone bridge, a similar wire being placed in the other branch. This method seemed to be quite sensitive, the results of the experiments leading the author to think that a bolometer strip of definite surface could be placed at a fixed distance from a carbon loop of known dimensions inside an exhausted glass vessel. The amount of radiation which the bolometer strip received being calculated, the standard of light might be based upon the point of incandescence which would give a definite radiation at a fixed distance. The energy produced by rays of different refrangibility could not be thus distinguished, but variations in the amount of energy received were indicated by the bolometer strip, while the difference in colour of the light of the glow lamp made the observer at the photometer entirely uncertain of his measurements.

The author next compared the radiation from a platinum strip with that from a thin brass vessel containing water, by means of a thermopile provided with the customary cones, a series of diaphragms of thick cardboard extending between the radiating surface of the vessel containing water and the platinum strip. This arrangement was extremely sensitive; a short coil galvanometer was connected with the thermopile, a movement of a centimètre in the position of the faces of the pile being sufficient to drive the spot of light from the galvanometer mirror off the scale, corresponding to a movement of nearly 50 centimètre scale divisions. One observer tested the light photometrically, whilst another observed the galvanometer, with the result that with similar photometric observations the thermopile indicated a large change in the amount of heat received.

In conclusion, the author thinks it possible to assume as a practical standard a carbon loop in an exhausted vessel raised to such a point of incandescence that it will radiate a definite amount of energy, this energy being measured by a bolometer strip or the thermopile at a definite distance from the carbon loop: this would have a greater range than an incandescent strip of platinum placed in free air. The latter method, however, for the incandescence which produces a light similar in colour to that of a sperm candle is extremely sensitive, and can be made more exact than present

photometric tests. Both methods have the great advantage of substituting a measure of energy for a relative indication by the eye, which is not connected with any absolute measurement.

G. LIPPMANN—AN ARRANGEMENT WHICH PERMITS OF ARRIVING AT THE MAGNETIC POTENTIAL OF A SYSTEM OF BOBBINS WITHOUT CALCULATION.

(*Comptes Rendus, Vol.* 100, *No.* 25, *June* 25, 1885, *p.* 1533.)

Usually, in order to determine the magnetic potential of a bobbin, it is necessary to know the dimensions of each turn of wire, and to perform a complicated calculation. Instead of one bobbin, suppose we take three similar ones (a, b, c), and arrange them at the points of an equilateral triangle in such a way that their axes form the three sides of an equilateral triangle (A B C). The variation of the magnetic potential due to this system, and taken from B to C, is exactly equal to the product $4 \pi n i$, where i is the strength of the current, and n the number of turns on each bobbin. To demonstrate that this is so, it is sufficient to point out that if we consider the integral of the magnetic action of the bobbin a, considered by itself, all round the periphery of the triangle A B C, this integral is exactly equal to $4 \pi n i$, because the periphery of the triangle is a closed line. On the other hand, the action of a on the side C A can be replaced by the action of c on the side B C; similarly, the action of a on the third side, B A, can be replaced by the action of b on the side B C; so that the action of the system of three bobbins on the side B C is equal to the sum of the actions of a on the three sides, *i.e.*, to $4 \pi n i$. The above reasoning can be extended to any regular polygon.

S. WROBLEWSKI—RESISTANCE OF COPPER AT − 200° C.

(*Comptes Rendus, Vol.* 101, *No.* 2, *July* 13, 1885, *p.* 160.)

Being desirous of testing experimentally the statement made by Clausius that the resistance of any chemically pure metal is proportional to its absolute temperature, and that hence at the absolute zero the resistance would also be zero and the conductivity infinite, the author undertook the experiments described.

The copper wires employed had a diameter of $\frac{4}{100}$ of a millimètre, were insulated with a double covering of silk, and were wound into small bobbins having at ordinary temperatures resistances varying from 3 to 20 Siemens units.

The intense cold requisite was obtained by making use of nitrogen boiling at its point of solidification. A preliminary series of experiments had shown

that liquid oxygen and nitrogen were perfect insulators. The results obtained are tabulated below.

Temp. Centigrade.	How obtained.	Bobbin I.		Bobbin II.	
		Resistance in S. U.	Coefficient.	Resistance in S. U.	Coefficient.
+ 100°	Boiling water	5·174	—	—	—
+ 21°·4	Air temperature	3·934	0·004365	—	—
± 0	Melting ice	3·614	0·004136	17·559	0·004057
− 103	Boiling ethylene	2·073	0·00414	9·848	0·004263
− 146	Critical point for nitrogen	1·360	0·004588	6·749	0·004104
− 193	Boiling nitrogen under atmospheric pressure	0·580	0·004592	2·731	0·004869
− 200	Solidification of nitrogen ...	0·414	0·006562	1·651	0·007688

A. GAIFFE—A STANDARD VOLT.

(*Comptes Rendus, Vol.* 101, *No.* 6, *August* 10, 1885, *p.* 431.)

In the course of some experiments made in 1872 with chloride of silver cells, the author found that the specific gravity of the chloride of zinc solution influenced the E.M.F., which was less for the heavier solutions. He concluded then that with a solution of 5 per cent. of chloride of zinc the E.M.F. is 1·02 B.A. volt, or 1·01 volt (C.G.S.).

Since then, further experiments have shown the author that certain irregularities previously noticed were due to impurities in the substances employed, and to changes of temperature, which have considerable influence as the temperature approaches zero.

Working with pure amalgamated zinc, pure fuzed chloride of silver, and clear solutions of pure chloride of zinc as nearly neutral as possible and at a temperature of 18° C., the same solution gives always the same E.M.F.; and a standard solution of density 1·07 gives exactly the legal volt. The author adds that such cells should never be used on a circuit of less than 5,000 ohms resistance.

[*Note.*—The density of 1·07 at 18° C. corresponds to about 8% of Zn Cl_2. 1·091 corresponds to 10% at 19·5° C., according to Kremers.]

W. SIEMENS—CONTRIBUTION TO THE THEORY OF MAGNETISM.

(*Journal de Physique, Vol.* 4, *Sept.*, 1885, *p.* 426.)

Starting from the ideas of Ampère on magnetism, the author arrives at the conclusion that magnetic bodies, like iron, oppose a weaker resistance to the magnetic polarisation than non-magnetic bodies, or that the former have a higher magnetic conductivity. A homogeneous ring, concentric with a conductor, cannot produce any action at a distance, since all the lines of force

remain inside the ring. In an open ring the total magnetism should be less on account of the higher resistance of the air in the opening, and this the author has verified experimentally.

Dr. Siemens generalises the law of Ampère and extends it to all bodies, thus: Not only magnetic bodies, but all bodies whatsoever, as well as empty space, are full of pre-existent circular currents of very small dimensions, and magnetic bodies only differ from non-magnetic bodies in that the number of these circular currents contained in the unit volume of the former is much greater than in the case of the latter.

All magnetic phenomena may be reduced to the property possessed by an electric current of exercising a directive action on the molecular solenoids spread throughout all space, and in greater number in magnetic bodies. By this directive action the axes of the molecular solenoids tend to place themselves at right angles to the direction of the current, and to form concentric circles of attraction. The greatness of the rotation of the axes depends on the value of the directive or magnetising force, and on the number of molecular currents contained in the unit of volume. The author recommends that this numeric relation should be styled the magnetic conductivity or resistance to magnetic distribution.

Experiments were made with horse-shoe magnets with and without keepers, and it was found that in the case of the closed magnetic circuit the magnetism increases at the beginning more rapidly than the strength of the magnetising current. In the case of the open magnet, without keeper, the magnetism for a weak current ($\frac{1}{20}$ ampère) is only about $\frac{1}{5}$ of that produced with the same current in a closed magnet, and for $\frac{1}{10}$ ampère it is about $\frac{1}{8}$. In the case of the open magnet the increase of the magnetism remains constant, *i.e.*, it is proportional to the current up to a certain limit. By reducing the length of the legs of the magnets, which projected beyond the coils on them, it was found that the magnetism in the case of the closed magnet increased considerably, whilst that of the open magnet is diminished still more considerably, so that instead of being $\frac{1}{5}$ for $\frac{1}{20}$ ampère, it becomes $\frac{1}{14}$, and $\frac{1}{23}$ for $\frac{1}{10}$ ampère.

Experiments were also undertaken to prove the relative values of the magnetic conductivity of iron and of air, with the result that the former is 480 to 500 times greater than the latter. This difference is not due to the action of the oxygen of the air—which is itself magnetic—as was determined by experiments with air, oxygen, hydrogen, or a vacuum between the poles of the magnets examined; the result being the same in all cases. A further experiment was made to prove that space filled with non-magnetic matter, as well as vacuum, can be influenced by electric currents, just as iron is influenced, only the effect is about 500 times less.

According to the author, when a current flows in a conductor wound on a bar of iron, there is produced in this latter as much magnetism as can be got into it by the sum of the magnetic moments of the elements of air which touch its surface. For the proof of this statement, as well as for details of the numerous experiments, the reader is referred to the original paper. The

various experiments are sufficient, in the author's opinion, to justify the hypothesis that there is no such thing as free magnetism; and that a magnetising force cannot produce in magnetic bodies more magnetism than can be fixed inside these bodies, as well as in the surrounding medium, by the action of the magnetic distribution in the form of closed curves producing in each section the same magnetic moment.

The magnetic distribution in a bar of finite length when the magnetising force acts symmetrically on all parts of the bar completely loses its logarithmic character, and assumes a parabolic form, as has been shown by Van Rees

In another paper, the author has worked out the idea that, according to Ampère's theory, the molecular magnets must be looked upon as being each formed of two elementary or solenoidal magnets placed face to face, with unlike poles. These two elementary magnets turn freely together in all directions without experiencing any resistance, but under the influence of an external magnetising force each little magnet would turn with respect to its neighbour in such a manner that both take up similar positions. The conclusions to be drawn from the above ideas would tend to enlarge Ampère's theory, and to the admission that the whole universe is full of molecular solenoids coupled together, or, if we admit the truth of Mr. Edlund's ideas, of whirls of ether; and the number of these whirls will be greater in magnetic matter than in non-magnetic. The magnetic moment produced by a magnetising force will be not only a direct effect of the current, but especially the product of the mutual action of the elementary magnets contributing to a rotation set up by the current. The author sees that this hypothesis will lead to a difficulty in accounting for the return of magnets to their initial state after the magnetising force is withdrawn and when there is not any coercive force; he explains it away, however, by a simultaneous effect of forces of attraction and of repulsion, which will lead to a state of equilibrium consistent with the results of experiments.

C. ELSASSER—TELEPHONIC INDUCTION.

(*Bulletin de la Société Internationale des Electriciens, July*, 1885, *p*. 210.)

The inconveniences arising from induction on telephone lines are specially noticeable when two exchanges have to be connected by a trunk line. When the two districts are far apart, the number of wires is reduced to a minimum to avoid the expense, and conversation carried on over one wire can be heard on another.

The German Post and Telegraph Department have tried all the various means proposed to get rid of this inconvenience, such as crossing the wires at each post; attaching to each post a special wire which is put to earth at various points; the use of induction coils in the circuit arranged in such a way that the currents induced in them should be in opposite sense to those induced in the line wires; the use of a complete metallic circuit, the earth not being used as a return. This latter method has been the most successful, especially when in

the case of three wires the two looped wires were in every place arranged symmetrically with respect to the third.

In some experiments on the line from Bremen to Bremerhaven—a distance of 43 miles—where there are four wires, the wires No. 2 and No. 3 on opposite sides of the posts were looped together to form a complete metallic circuit, whilst the wires No. 1 and No. 4 (top left and bottom right) were used as single conductors for the telephonic transmissions of the subscribers. The induction, however, was so great that it was impossible to work the line in this way, and translating induction coils had to be introduced into the subscribers' lines. The ordinary forms of induction coils so weakened the sounds transmitted that conversation was very difficult. Much better results were obtained by adopting a suggestion of Mr. Landrath to use induction coils in the form of a horse-shoe magnet with a keeper, it being found that the induced currents were 1·7 times more powerful with these coils than with coils without a closed magnetic circuit.

Difficulties occur where these induction coils are introduced on circuits of more than two wires; they can, however, be got over by a suitable disposition of the wire on the posts. If there are eight wires, then, reckoning from the top wire downwards alternately on one side and on the other of the post, wires No. 1 and No. 4 would be looped, as well as wires No. 2 and No. 3; No. 5 and No. 6 would also be looped, but in addition they would be crossed at half the length of the line, while the looped wires No. 7 and No. 8 would be crossed three times.

Turning from aerial lines to underground ones, the author cites the cables made by Siemens and Halske, and by Felten and Guilleaume. The former adopt the plan of wrapping each insulated wire with a few bare copper wires, which can, if necessary, be used as return wire, or they may be all joined up together and put to earth. The latter wrap each insulated wire in sheets of tinfoil, the whole number of strands thus formed being bound together by bare copper wires, which are thus in electrical connection with the tinfoil, and also with the lead covering of the cable.

E. BUDDE—ON THE THEORY OF THERMO-ELECTRIC FORCES.

(*Annalen der Physik und Chemie, Vol.* 25, *Pt.* 4, 1885, *p.* 564.)

In a former paper the author arrived at the conclusion that the translation theory is dependent on the condition that all thermo-elements consisting of two conductors must obey the law of Avenarius. This conclusion is not incorrect, but it does not go far enough. If two metals, a and b, are joined up to form a thermo-element, if D_a and D_b are their temperature co-efficients, T_1 and T_2 the temperatures of the junctions, then, according to Kohlrausch's theory, the E.M.F. of the element is

$$E = -\int_{T_1}^{T_2} D_a \, d\,T - \int_{T_2}^{T_1} D_b \, d\,T.$$

If we now substitute

$$D_a = -a\,T \quad D_b = -b\,T,$$

where a and b are constants, we have

$$E = \int_{T_1}^{T_2} a\,T\,d\,T + \int_{T_2}^{T_1} b\,T\,d\,T$$

$$= \tfrac{1}{2}\,(a - b)\,(T_2^2 - T_1^2).$$

The translation theory leads, therefore, to an expression for E in which only the squares of the end temperatures occur. This result is now, in the present state of observations, not any more merely improbable, like the law of Avenarius, but it is certainly in contradiction to experience. This can be seen at once without any calculation; for let us take T_1 as the zero point of an abscissa on which the temperatures T_2 are to be set off, and let us plot the corresponding values of E as ordinates, then it follows from the last equation that the second differential coefficient of E must always have the same sign as the first; hence for $T_2 > T_1$ all curves would turn their convex side to the axis of the abscissæ. A glance at the results of Gaugain and others shows that this is not the case. The author therefore concludes that it is unnecessary to go into the other points raised by Kohlrausch, but he remains of his former opinion that the translation theory is untenable. This theory owes its existence to Kohlrausch's assumption that the junctions are inoperative. If we take the formulæ given by Kohlrausch for the E.M.F., and convert them on the assumption that the junctions are of influence in producing the result, we arrive at the theory of W. Thomson. The author's and Thomson's theories are only two ways of expressing the same thing, due to their each making use of a different symbol to characterise the behaviour of the conductor.

S. ARRHENIUS—RESEARCHES ON THE CONDUCTIVITY OF ELECTROLYTES.

(*Beiblätter, Vol.* 9, *No.* 6, 1885, *p.* 437.)

The current from a Daniell battery was joined through a key to a differential galvanometer, one branch of which was connected to rotating commutator, by means of which the current could be reversed 24 times a second. From the commutator the current passed by a switch either through the liquid resistance to be measured or through a box of resistance coils. The second branch was connected to a resistance or to a column of sulphate of zinc between amalgamated zinc electrodes. The liquid under test was first balanced against the resistance in the second branch, and then, the liquid being replaced in the first branch by the box of resistances, this was balanced against that in the second branch. The resistance of the liquid was then equal to that which had been substituted for it.

It is only possible to glance at some of the results obtained by the author, and stated in the paper, which is of considerable length. The author gives a

table of numbers, which show the ratio in which the coefficients of conductivity are diminished by diluting the liquid to double the quantity; these numbers he calls the "solution coefficient." For instance, taking caustic soda, between the conductivities 33 and 3·87 this ratio is 2·18 to 1.

All solutions of electrolytes in water have a "solution coefficient" which is less than 2.

The "solution coefficient" generally increases with the dilution, with the exception of some hydrates and carbonates.

The "solution coefficients" of salts belonging to the same groups are very nearly alike, *e.g.*, potassium, sodium, and ammonium salts; chlorides, bromides, and iodides.

The conductivity of a solution is, *cæteris paribus*, proportional to the quantity of salt dissolved.

In a solution of two salts (which do not react on each other) the conductivity of the mixture is equal to the sum of the conductivities of the individual solutions; and the conductivity of a solution where the solvent is itself a conductor is equal to the sum of the conductivities of the solvent and of the salt.

If the conductivity of a solution does not alter in proportion to the quantity of salt dissolved, chemical action has taken place.

The author brings forward several theories more or less closely connected with the chemical properties and behaviour of electrolytes, into which it is not necessary to follow him.

A. BARTOLI—CONDUCTIVITY OF CARBON COMPOUNDS, SPECIALLY OF AMIDES, ANILIDES, AND NITRO-DERIVATIVES.

(*Beiblätter, Vol.* 9, *Pt.* 10, 1885, *p.* 681.)

The badly conducting liquids were placed in a test glass 200 mm. high and 30 mm. diameter, between two parallel platinum electrodes fused into the glass, with a space of 10 mm. between them; the better conductors were placed in U tubes, with platinum plates as electrodes.

The results obtained show that the amides and anilides—formamide, acetamide, propionamide, benzamide, bromamido-naphthaline, mono-, bi-, and tri-acetimide, bi- and tri-bromacetamide, formanilide, acetanilide, and benzanilide—in the liquid state conduct very well, and their coefficient of conductivity increases with the temperature; some conduct even after solidification, but the conductivity decreases in proportion as the temperature falls below the point of solidification. The amines of the aromatic series behave in the same way, such as phenylamine, diphenylamine, ethyaniline, paratoluidine, &c., &c.

The nitro-derivatives are also conductors, and the conductivity of an compound is increased in every case by the combination of the radicle NO_2. Liquid carbon compounds, mixed or dissolved, increase their conductivity with the temperature.

A. BARTOLI—CONDUCTIVITY OF SOME SOLID ORGANIC BODIES.

(*Beiblätter, Vol.* 9, *Pt.* 10, 1885, *p.* 682.)

In the case of some organic compounds a sudden and considerable decrease of conductivity occurs at the moment of solidification, and if the temperature be lowered a few degrees more they cease to conduct altogether; such bodies are nitrotoluene, dimethylaniline, diphenylamine. Others retain their conductivity until a much lower temperature is reached; such are benzamide, phenol, paratoluidine.

Although very small admixtures may render a body a conductor, since they solidify far below the latter, yet the author is not of opinion that such admixtures can influence the phenomena.

A. BARTOLI—CONDUCTIVITY OF THE RESINS.

(*Beiblätter, Vol.* 9, *Pt.* 10, 1885, *p.* 683.)

The resins show no peculiarities on heating in the neighbourhood of the melting point. They are all perfect insulators for a long distance below the point of solidification, and become conductors after the point of softening or of melting is reached; their conductivity almost always increases proportionally with the temperature. The more acid and oxidised constituents the resins contain the better they conduct, and *vice versâ*. The following are some of those which conduct well in a molten or soft state:—Styrax, jalap, scammonin, dragon's blood, amber; the balsams of Peru, of tolu, and of copaiba; shellac, assafetida, laudanum, aloes, myrrh, Venetian turpentine.

The following are moderately good conductors above the melting point:—Chios turpentine, ordinary pitch, colophonium, asphalt. Others only conduct very little; such are Canada balsam, copal, mastic, dammar.

In this case also the viscosity seems to have influence, and as it increases the conductivity decreases. Thus, if with guaiacum an equal quantity (or four times the quantity) of naphthaline is mixed, which is an absolute non-conductor, and melts at just the same temperature as the resin, the mixture conducts much better than the guaiacum resin alone, although it is much less viscous.

A. BARTOLI—CONDUCTIVITY OF MIXTURES OF ORGANIC COMPOUNDS.

(*Beiblätter, Vol.* 9, *Pt.* 10, 1885, *p.* 683.)

Certain mixtures and solutions of organic compounds increase in conductivity on solidification, and retain their conducting properties for many degrees below this point; such are naphthaline with nitro-naphthaline, paraffin with a small quantity of amylic alcohol, formic acid, or acetic acid, &c. Animal and vegetable fats and oils do not behave thus. This behaviour the author explains in the following way:—If a non-conducting solid substance is

mixed after being melted with a liquid, in which it is not dissolved, as, for instance, paraffin with amylic alcohol or acetic acid, one part of the latter can be distributed amongst the latter, and the mass conducts badly independent of the temperature; but on solidification of the paraffin the molecules of the liquid come into immediate contact with each other between the paraffin molecules, and the mass becomes a conductor.

If the non-conductor dissolves in the conductor, and if this latter is present in very small quantity, the mixture has two melting points, one near that of the preponderating substance, and the other at a lower temperature. After solidification of the non-conducting substance, the portion of the mixture which still remains liquid is rich enough in the conducting body to be itself a conductor. If this last portion of the conducting body also solidifies, the whole mass becomes a non-conductor.

V. von LANG—METHOD OF MEASURING THE E.M.F. OF THE ELECTRIC ARC.

(Centralblatt für Elektrotechnik, Vol. 7, No. 22, 1885, p. 443.)

The method used was one which had been adopted by the author for the measurement of the internal resistance of a battery. An even number ($2n$) of similar cells are connected in series, and the circuit completed through a resistance. The point in this resistance is then found, which has the same potential as the middle point of the battery at the junction of the n^{th} with the $(n+1)^{\text{th}}$ cell. Having found this point, the resistance of the two parallel halves of the circuit can be determined by means of a Wheatstone bridge.

In the actual experiments the main circuit consisted of a battery of 58 medium Bunsen cells in series, the first lamp, an ammeter, eleven resistances of one-tenth of an ohm each, the second lamp, and so back to the battery. The connections to the bridge were made on one side to the junction between the 29th and 30th cells, and on the other to a commutator which could make contact with the junction between any two of the resistances of one-tenth of an ohm. The lamps were hand-regulated, the one by Professor F. Exner, the other by Dr. E. Lecher, and an image of the carbon points was projected on a screen by a lens. Carbons 5 mm. in diameter were used, and the distance between the points was 0·3 mm. The bridge used was of the usual stretched wire pattern. Twelve measurements were made at such moments as the two arcs were burning steadily and quietly without hissing. The average current was 4·33 ampères. The mean resistance of the parallel circuits was 1·82 ohms. This had to be diminished by the resistance of the leads to the bridge, and then multiplied by two to obtain the resistance of half the battery, plus one lamp, plus half the main circuit. All calculations made, the value of the E.M.F. deduced from resistance, multiplied by current, came out as 39 volts.

Dr. O. FRÖLICH—THEORY OF THE DYNAMO MACHINE.

(*Elektrotechnische Zeitschrift, Vol.* 6, *No.* 3, *March*, 1885, *p.* 128.)

The theory of the dynamo machine admits either of an algebraical or of a graphical treatment.

The graphical theory is founded on the curve, called the "characteristic" by Deprez, and the "curve of magnetism" by me, and is usually restricted to direct graphic deductions from this curve. As is well known, the characteristic is obtained, if for a constant speed, varying currents are produced by the dynamo, and the E.M.F. is plotted as ordinate, the current as abscissa. To obtain the curve of magnetism, observations at various speeds may be employed, since it is not the E.M.F. which is plotted as ordinate, but the ratio of E.M.F. to speed. The two curves only differ by constant factors; but there is only one single curve of magnetism for one dynamo, while there are any number of characteristics, one for each speed. These curves only show directly the action of the electro-magnets, and the way in which the magnetism is effected; the action of the armature is only shown indirectly.

These curves alone will not answer all questions about a given dynamo; they do not tell us directly anything about the relation between the current and E.M.F., for instance, and the speed and external resistance. To solve this question we must introduce what I call "current curves." By putting together the curves for magnetism and for current, the dynamo is completely characterised, and a complete graphic theory is arrived at, which will be true for any kind of connection.

Some further knowledge is, however, still necessary, viz., how the dynamo will behave with various kinds of windings, and how the electrical quantities will be affected by changes in the dimensions of the dynamo. This further knowledge must be sought in the algebraical theory.

A complete theory has been given by Clausius, but his formulæ are too difficult to be readily handled, and something more simple is requisite.

My theory depends on Ohm's law, on the law of induction in a magnetic field, and on the law, which has been proved experimentally, viz., that the so-called "current curve" of a dynamo departs very little from a straight line. For this last law a simple formula can be deduced for the relation between "magnetism" and "current."

As the essential purport of this theory, the formula for the magnetism is very frequently given; in reference to this statement I desire to remark—

1. That in this theory for the first time the relation between current, speed, and resistance for a series dynamo is established—a relation which, in spite of its simplicity, has been omitted by those who have formerly treated of this subject.
2. That this theory is the only one with which for a certain dynamo an agreement between theory and experiment has been obtained for its behaviour under all conditions.

Although in what follows I assume an acquaintance with my theory, I must

recapitulate the fundamental formulæ, in order to bring them into a different shape, which will render them more easily understood.

The electro-motive force (E) of any dynamo is proportional to the speed (v) and to the force of the magnetic field (M), so that

$$\mathrm{E} = \mathrm{M}\, v.$$

$$\text{From Ohm's law, } \mathrm{I} = \frac{\mathrm{E}}{\mathrm{R}};$$

$$\text{hence } \mathrm{I} = \frac{\mathrm{M}\, v}{\mathrm{R}}.$$

Since the strength of the magnetic field depends only on the current (I), by dividing both sides of this equation by M we get on the left side a function of I, and on the right $\frac{v}{\mathrm{R}}$; hence the current depends only on the ratio of the speed to the resistance.

But experience shows that if we plot these ratios as abscissæ and the currents as ordinates, the current curve does not differ materially from a straight line, which does not pass through the origin; hence we have

$$\mathrm{I} = c\frac{v}{\mathrm{R}} - d,$$

where c and d are constants.

Eliminating from the above two equations the quantity $\frac{v}{\mathrm{R}}$, we get for the strength of the magnetic field an expression of the form

$$\mathrm{M} = \frac{\mathrm{I}}{a + b\,\mathrm{I}},$$

where a and b are constants.

This point of view is true not only for series dynamos, but also for shunt and compound ones.

If we calculate the constants a and b for various machines, we obtain different values, since these contain a certain constant for the armature besides magnetic quantities. This combination of constants is injudicious; in fact, the strength of the magnetic field is a quantity, which indeed depends on the arrangement of the iron in the machine, on the winding, and on the claims of the machine, but only on their relative, not on their absolute, values. If a regular series of machines are constructed of various sizes, the dimensions of which are increased in a regular ratio, which have therefore similar geometric shapes, and if we consider each machine in the same proportion to its size, then similar machines show the same strength of magnetic field.

It is evident, therefore, that it is natural to measure the strength of the magnetic field, not as an absolute value—for example, as the number of lines of force cut by the wires on the armature—but as a relative value, *i.e.*, as the degree of magnetism. This we obtain if we put the maximum of the magnetic field equal to unity, *i.e.*, in the above formulæ for M, $b = 1$. Hence

$$\mathrm{M} = \frac{\mathrm{I}}{a + \mathrm{I}} = \frac{\frac{1}{a}\mathrm{I}}{1 + \frac{1}{a}\mathrm{I}}; \text{ or, if } m = \frac{1}{a},$$

$$\mathrm{M} = \frac{m\,\mathrm{I}}{1 + m\,\mathrm{I}} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (1)$$

The value m is proportional to the number of turns of wire on the electro-magnet, or more nearly, perhaps, to a certain power of this number; the strength of the magnetic field depends only on the product of the quantity m multiplied by I; taking this product as abscissa, and the strength of the magnetic field as ordinate, we obtain for any dynamo always the same curve. The degree of magnetisation of a machine is shown by the corresponding points of this curve, and shows, if the machines are similar, in the above method of reckoning always the same number, *e.g.*, 0·72—that is, 72 per cent. of the maximum strength of the magnetic field.

The curve of the magnetic field is characterised by the fact that at the commencement it rises nearly in a straight line, then turns sharply to the right, and approaches at an infinite distance as an asymptote a horizontal straight line. I propose to call the point where the change of direction is most marked the "elbow" of the curve.

For practical purposes we only need to use the curve near its "elbow," since we make use neither of a very weak magnetic field (which would mean that the dynamo would produce only a very small current), nor of a very strong field (which would lead to too great an expenditure of power to produce it).

It is not to be forgotten that in reality the curve of the magnetic field does not gradually increase up to the value 1, but for large currents falls off past the elbow, this falling off being due to the action of the current in the armature. This complication, however, need only be taken into account when we are dealing with very large currents.

In modern times a variety of methods of winding dynamos has been introduced, and the magnetic field is due to a combination of these windings; and we have to find the total magnetic field M, if the partial fields M_1 and M_2 are given.

The following formula will give this value:—

$$1 - M = \frac{(1 - M_1)\ (1 - M_2)}{1 - M_1 M_2} \quad \dots \quad \dots \quad (2)$$

e.g., if the individual magnetic fields—*i.e.*, the fields which would be produced by either set of coils acting separately—are $M_1 = 0{\cdot}60$, $M_2 = 0{\cdot}25$, then $M = 0{\cdot}65$. Assuming that $M_1 + M_2 = 1$—*i.e.*, that one is always the complement—then, if we plot M_1 as abscissæ, M as ordinates, we obtain a curve from which we see that on the whole we obtain a more powerful field, the more powerful one of the individual fields is, thus:—

$$\text{If } M_1 = \tfrac{1}{2},\ M_2 = \tfrac{1}{2} \therefore M = 0{\cdot}67;$$
$$\text{if } M_1 = \tfrac{3}{4},\ M_2 = \tfrac{1}{4} \therefore M = 0{\cdot}77;$$
$$\text{if } M_1 = 1,\ M_2 = 0 \therefore M = 1{\cdot}00.$$

If we put the value of M from formula (1) into the equation for the current, we obtain—

$$I = f\frac{v}{R} - \frac{1}{m} \quad \dots \quad \dots \quad \dots \quad \dots \quad (3)$$

in which f is a constant, which has to be introduced in place of the constant b.

Both terms on the right are currents: $f\frac{v}{R}$ is the maximum current (I^1),

that is, that current which would be produced for a speed (v) and a resistance (R if the maximum magnetic field was at work; $\frac{1}{m}$ is that current which must circulate in the windings of the magnets to give rise to the magnetic field $\frac{1}{2}$.

The meaning of the two terms becomes still clearer in the "current curves" which we obtain by plotting the ratio $\frac{v}{R}$ as abscissæ, I as ordinates, and which except at its commencement, corresponds with a straight line which does not pass through the origin. If now we draw a line through the origin parallel to this straight line, we have the line, the equation of which is

$$I^1 = f\frac{v}{R},$$

or the line of the maximum current; hence for a given value of $\frac{v}{R}$ we can always obtain the real current when we subtract from the corresponding value of the maximum current the value of the current necessary for producing the magnetic field $\frac{1}{2}$.

We can find, therefore, for a dynamo with any method of winding, any electrical quantity (except work) by looking at the question from two points of view—1st, when the maximum magnetism is produced, and 2nd, when the current in the electro-magnets has such a value that the magnetic field becomes $\frac{1}{2}$; we calculate the corresponding electrical quantities for each, and the difference of the two is then equal to the real value sought. By means of this law, moreover, we separate every electrical quantity into a quantity belonging to the armature and a quantity belonging to the electro-magnets.

I propose to return later to this question of the relative parts played by the armature and the electro-magnets in these equations, and I will give now the formulæ for difference of potential, E.M.F., and current, for various types of machines:—

Let E = the E.M.F.;

I_a, I_s, I = the currents in the armature, in the shunt circuit, and in the outer circuit;

P = the difference of potential at the terminals;

a, s, p = the resistances of the armature, the series coils, and the shunt (parallel) coils;

R = the outer resistance;

m_s, m_p = number of turns in the series and shunt coils respectively.

A. Series Dynamo.

$$I_a = I = \frac{fv}{a+s+R} - \frac{1}{m_s}$$

$$E = fv - \frac{1}{m_s}(a+s+R$$

$$P = \frac{fv}{1+\frac{a+s}{R}} - \frac{R}{m_s}$$

B Shunt Dynamo.

$$I_a = \frac{f v}{a + \frac{p R}{p + R}} - \frac{1}{m_p} \cdot \frac{R + p}{R}$$

$$I_p = \frac{f v}{a \cdot \frac{R + p}{R} + p} - \frac{1}{m_p}$$

$$I = \frac{f v}{a \cdot \frac{R + p}{p} + R} - \frac{1}{m_p} \cdot \frac{p}{R}$$

$$E = f v - \frac{1}{m_p}\left(a \cdot \frac{R + p}{R} + p\right)$$

$$P = \frac{f v}{1 + a\left(\frac{1}{p} + \frac{1}{R}\right)} - \frac{p}{m_p}$$

C. Compound Dynamo.

1 *Shunt in parallel with outer circuit.*

$$I_a = \frac{f v}{a + s + \frac{R p}{R + p}} - \frac{\frac{R + p}{R p}}{\frac{m_p}{p} + m_s\left(\frac{1}{R} + \frac{1}{p}\right)}$$

$$I_p = \frac{f v}{(a + s)\left(1 + \frac{p}{R}\right) + p} - \frac{\frac{1}{p}}{\frac{m_p}{p} + m_s\left(\frac{1}{R} + \frac{1}{p}\right)}$$

$$I = \frac{f v}{a + s + R\left(1 + \frac{a + s}{p}\right)} - \frac{\frac{1}{R}}{\frac{m_p}{p} + m_s\left(\frac{1}{R} + \frac{1}{p}\right)}$$

$$E = f v - \frac{1 + (a + s)\left(\frac{1}{R} + \frac{1}{p}\right)}{\frac{m_p}{p} + m_s\left(\frac{1}{R} + \frac{1}{p}\right)}$$

$$P = \frac{f v}{1 + (a + s)\left(\frac{1}{R} + \frac{1}{p}\right)} - \frac{1}{\frac{m_p}{p} + m_s\left(\frac{1}{R} + \frac{1}{p}\right)}$$

2. *Shunt in parallel with armature.*

$$I_a = \frac{f v}{a + \frac{p\,(R + s)}{p + R + s}} - \frac{\frac{p + R + s}{p\,(R + s)}}{\frac{m_p}{p} + \frac{m_s}{R + s}}$$

$$I_p = \frac{f v}{a \cdot \frac{p + R + s}{R + s} + p} - \frac{\frac{1}{p}}{\frac{m_p}{p} + \frac{m_s}{R + s}}$$

$$\mathrm{I} = \frac{f\,v}{a + (\mathrm{R} + s)\dfrac{a + p}{p}} - \frac{\dfrac{1}{\mathrm{R} + s}}{\dfrac{m_p}{p} + \dfrac{m_s}{\mathrm{R} + s}}$$

$$\mathrm{E} = f\,v - \frac{1 + \dfrac{a}{p} + \dfrac{a}{\mathrm{R} + s}}{\dfrac{m_p}{p} + \dfrac{m_s}{\mathrm{R} + s}}$$

$$\mathrm{P} = \frac{f\,v}{\left(1 + \dfrac{s}{\mathrm{R}}\right)\left(1 + \dfrac{a}{p}\right) + \dfrac{a}{\mathrm{R}}} - \frac{\dfrac{\mathrm{R}}{\mathrm{R} + s}}{\dfrac{m_p}{p} + \dfrac{m_s}{\mathrm{R} + s}}$$

Numerous experiments with dynamos wound in various ways have proved the accuracy of the above formulæ. The only point on which I have still some doubt is whether the quantity m should be made proportional to the number of turns of wire on the magnets, or to some power of this number.

A table is given showing the actual results obtained with a large Siemens dynamo, compound wound, giving a current of about 150 ampères, with an E.M.F. of about 125 volts. An inspection of this table shows that the differences between the values actually measured and those obtained by calculation from the formulæ do not exceed one per cent.

The deduction of practical rules for winding dynamos from the above theory is not easy, on account of the complicated reckoning required; but many experiments may be saved in constructing dynamos if for any compound winding we determine the constants f, m_s, and m_p, and try to find out by calculation how m_s and m_p should be changed so as to obtain as straight a line as possible as representing the difference of potential at the terminals.

A question which has arisen is that of the change of difference of potential with the speed in simple-shunt and compound machines; a glance at the preceding formulæ will give the answer, viz., that this change is somewhat less with compound machines than with shunt machines. The same theory has also led to a combination of windings by which it is possible to maintain a constant current, which is very useful in electrolytical operations. This can be done by winding a very few series coils on the top of the shunt coils of a dynamo, the connections being so made that the current flows in the *opposite* direction in the series coils to what it does in the shunt coils: by this means the current may be kept constant although the external resistance may vary 40 per cent.

G. FOUSSEREAU—ELECTRIC RESISTANCE OF ALCOHOL.

(*Journal de Physique, Vol.* 4, *Oct.*, 1885, *p.* 450.)

The first experiments were made upon several samples of commercial absolute alcohol, with the result that the specific resistance at 15° C. varied from 2·47 megohms to 3·68 megohms. The wide differences between the values obtained were too great to be attributed to errors of observation; and, bearing

in mind his experiments on distilled water in which minute quantities of salts were dissolved, the author proceeded to the investigation of the results of such additions to alcohol.

Successive quantities of water were added to the absolute alcohol, and it was found that the resistance continually decreased as more and more water was added, until there was only three per cent. of alcohol in the mixture, after which further additions of water caused an increase in the resistance. It was soon apparent, however, that the presence of such a large quantity of water was necessary to cause any considerable decrease of resistance, that the small quantities found in commercial absolute alcohol could not be sufficient to account for the different values obtained in the first experiments.

The slightest trace of any salt, inappreciable by any ordinary tests, has a most marked effect on the resistance; acids have the same effect. The effect was best shown in the case of caustic potash, thus—

Proportion of KHO.		Resistance.
0		1
$\frac{1}{1320000}$	... ...	0·295
$\frac{1}{86100}$	... ...	0·0348
$\frac{1}{2540}$	... ...	0·0016

Seeing this result, it was to be expected that the resistance of alcohol would be affected by the gradual solution of traces of the alkalies contained in the glass of the vessels in which it was kept; and the author found that such was the case, while vessels of porcelain had little or no effect. One experiment was made with a sample of alcohol which had been prepared with every possible precaution, and kept in a stoppered bottle for two years. The specific resistance was 0·278 megohms—a result from 9 to 13 times less than had been obtained with commercial absolute alcohol. If the alcohol is heated above 40° C., the action is still more marked.

Since slight impurities decrease the specific resistance of the alcohol, it is interesting to find out what is the maximum resistance, since we may then conclude that we have also the maximum degree of purity. To do away with the effect of the glass the alcohol experimented upon was specially distilled in a copper still, and received in porcelain vessels. One sample collected in unglazed porcelain had a specific resistance of 5·44 megohms, which had only fallen to 5·24 megohms at the end of six days, showing that scarcely any salt had been dissolved out of the porcelain. Another sample, repeatedly purified over lime and anhydrous baryta, gave the value 7·031 megohms, which by two hours' contact with a glass bottle fell to 2·823 megohms. This result was checked a few days later with a fresh quantity of alcohol, collected in porcelain, and the value 6·899 megohms was obtained.

The effects of alteration of temperature were also investigated, though with difficulty, owing to the alcohol dissolving out some of the alkali from the glass vessels. The resistances R_t and R_{t^1} for the two temperatures t and t^1 are connected by the equation

$$R_t = R_{t^1} \times a^{t^1 - t}.$$

The value of the coefficient a was determined for the various experiments, and was found to be for

$$- 19{\cdot}5^\circ \text{ to } + 17^\circ = 1{\cdot}0156;$$
$$0^\circ \text{ to } + 15^\circ = 1{\cdot}0147;$$
$$+ 12^\circ \text{ to } + 24^\circ = 1{\cdot}0135.$$

The author has established the fact that for distilled water and some fused salts the resistance varies as the coefficients of internal friction; this, however, does not hold in the case of alcohol, the coefficient of friction of which varies much more rapidly than the resistance.

P. H. LEDEBOER—MEASUREMENT OF SMALL RESISTANCES.

(*La Lumière Electrique, Vol.* 17, *No.* 27, *July* 4, 1885, *p.* 1.)

The usual method is by the double bridge plan due to Thomson, and the author considers what are the best conditions under which the measurements may be made, assuming that for ordinary purposes we may admit an error of one per cent., or, in very exact cases, of one in a thousand; indeed, the lineal measurements of the dimensions of the bar or rod will not admit of a closer approximation than this. The method of measurement itself is too well known to need description, and it will suffice to glance at the conditions recommended. From the formula for the current in the bridge it appears that when the resistance to be measured is very nearly equal to the standard of comparison the auxiliary resistances should be small. It is also advisable to have as large a difference of potential as possible at the ends of the resistances; this being, however, limited by the fact that it is undesirable to heat the conductors. For nearly equal resistances good conditions are obtained if we make the auxiliary resistances about 1 or 2 ohms, and the resistance of the galvanometer from 4 to 10 ohms.

In the case where the standard resistance is greater than the resistance to be measured there is not the same objection to the auxiliary resistances being large—they may, indeed, be considerably more than that of the galvanometer—but, on the other hand, owing to the small differences of potential, the galvanometer must be highly sensitive, unless we make use of mercury or German silver wire as the standard of comparison, when, owing to the temperature coefficient being very small, we may use higher differences of potential.

Another method of measuring very small resistances is with the differential galvanometer, as has been described by Kohlrausch, who shows a way of eliminating the small errors due to any dissymmetry in the two bobbins. The same considerations as to the E.M.F., the resistances, &c., to be used, hold good in this case also.

P. MARCILLAC—THE DRY PILE OF PROFESSOR PALMIERI.

(*La Lumière Electrique, Vol.* 18, *No.* 41, *Oct.* 10, 1885, *p.* 69.)

Owing to the construction of Bohnenberger's electrometer, the question of a good dry pile which should not be affected by the hygrometric state of

the air has become of some importance. Dry piles are very long-lived—one made by Zamboni fifty years ago, and which is in the possession of Palmieri, still shows traces of action—but as usually constructed, with the discs in close contact with the glass tube in which they are placed, they lose much of their power in damp weather.

In the form of dry pile imagined by Palmieri the discs of paper are covered on one side with tinfoil, and on the other with peroxide of manganese rendered coherent by milk. A great number of these discs are piled on the top of each other, and are placed on a metal base having three horizontal projecting arms. Surrounding the pile of discs, but at a distance of some millimètres, is a glass cylinder, thus allowing an air space all round the pile; on the top of the pile is a brass plate against which presses a set screw, which passes through a nut with three horizontal arms. These three arms of the nut are connected to the three arms of the base by three silk cords, and thus the pile can be compressed as desired. The only thing necessary is to dry the silk cords from time to time, and the pile is then most perfectly insulated.

J. ELSTER and H. GEITEL—UNIPOLAR CONDUCTIVITY OF HEATED GASES.

(*Annalen der Physik und Chemie, Vol. 26, Pt. 1, No. 9, 1885, p. 1.*)

The object of this communication is to show that the theory put forward by Herwig with respect to the unipolar conductivity of gas flames is founded on the phenomenon observed by the authors, that electricity is produced so soon as gases come into contact with incandescent bodies. Their results may be summed up thus: Every flame may be regarded as a current of heated gas, which produces negative electricity in incandescent electrodes introduced into it, as well as in the solid particles in it; hence it is probable that the incandescent particles of gas themselves may be considered as playing the part of the electrode towards the cooler ones. The observations of several experimenters—amongst others, Hankel and Guthrie—agree in the conclusion that the interior of a flame is a positive unipolar space.

The authors have repeated some of the former experiments, including those of Guthrie, using for the purpose the Paquelin burner. This consists of a hollow platinum sphere filled with spongy platinum, inside which a mixture of vapour of benzine and air is burnt. The experiments fully confirmed Guthrie's observation that the conductor is much more rapidly discharged when it is negatively electrified than when it is positively electrified. The discharge by the incandescent platinum sphere still takes place at a distance of half a mètre and more, when of course there can be no longer any question of true conductivity of the air; also the phenomenon of unipolarity disappears if the incandescent sphere which is put to earth is brought up to within 3 cm. of the conductor.

The authors also investigated the question of the behaviour of constant sources of electricity in the hot air surrounding the Paquelin burner, and

found that this hot air was negatively unipolar, in opposition to the flame. They also measured the potential, and give a table of values, from which the negative unipolarity of the hot air is very apparent. Some measurement made with large battery power were specially interesting as showing that the electricity developed at the incandescent sphere was sufficient to reduce to zero the potential at the negative pole of a battery of about 8·5 Daniells.

According to the authors the following is what happens in the case of unipolar conductivity in flames:—Luminous flames are full of a great quantity of carbon particles at a white heat, which are constantly negatively electrified with respect to the surrounding gas. If two oppositely charged electrodes are introduced into the flame, these negatively electrified particles are repelled from the negative electrode, and are attracted to the positive electrode, the positive charge on which is thus neutralised. The negative particles do not, however, lose their charge finally, but regain it owing to the E.M.F. acting between the heated gases and the solid incandescent bodies.

In the case of non-luminous flames, the explanation of the unipolar conductivity is attended with great difficulties, since it must be assumed either that they also contain a sufficient number of solid particles to account for the phenomenon, or else that the hotter particles of gas behave towards the less hot in the same way as the solid particles in luminous flames do to the surrounding gas. The latter is, in the authors' opinion, the more probable.

W. v. BEETZ—GALVANIC DRY BATTERIES AND THEIR USE FOR ELECTROMETRIC AND GALVANOMETRIC MEASUREMENTS.

(*Annalen der Physik und Chemie, Vol.* 26, *Pt.* 1, *No.* 9, 1885, *p.* 13.)

The batteries described consist of U tubes, one half of which is filled with solution of copper sulphate, the other half with solution of zinc sulphate, both legs of the tube being filled with plaster of Paris; before this had set a copper wire was introduced into the one limb and a zinc wire into the other, and finally both ends were stopped up with paraffin. These cells having been under observation for some fifteen months, the author is in a position to give particulars of their behaviour. The E.M.F. of these cells was tested against a standard Daniell (1·059 volt), which was compared with a Latimer Clark cell (1·457 volt). Since Lord Rayleigh's latest determination of the Latimer Clark cell (1·434 volt) the E.M.F. has been redetermined, the standard Daniell used being taken as 1·177 volt as measured between 16° and 20°.

In the oldest cells (Group I.) the zinc was not amalgamated; in the newer ones the zinc wire is covered with shellac, with the exception of its extreme end, which was amalgamated. In the cells of Group II. the zinc and copper solutions were saturated at the ordinary temperature, whilst in Group III. they were diluted with half their volume of water. The solutions in Group IV. were concentrated by boiling, while in Group V. the so concentrated solutions had a portion of powdered salt added to them. In the last three groups, moreover, the tubes were almost entirely filled with plaster of Paris

moistened by distilled water, only about 4 cm. at each end containing the respective solutions. The values of the E.M.F. are tabulated below:—

Group.	Cell.	24/2/84.	13/5/84.	29/6/84.	20/9/84.	22/1/85.	9/3/85.	8/4/85
I.	1	1·047	...	1·045	1·049	...	1·050	1·045
	2	1·045	...	1·043	1·044	...	1·048	1·044
II.	4	...	1·059	...	1·054	1·055	1·056	1·050
	8	...	1·057	1·055	1·054	1·058	1·055	1·054
III.	19	...	...	1·066	1·068	1·067	1·064	1·069
	21	...	...	1·068	1·068	1·068	1·069	1·068
IV.	26	...	...	...	...	1·062	1·064	1·062
	28	...	...	...	...	1·064	1·065	1·062
V.	36	...	...	...	...	1·060	1·060	1·060
	39	...	...	...	...	1·061	1·061	1·063

From the above table it appears that the cells with non-amalgamated zinc have the smallest E.M.F., those with dilute solutions the highest. For constancy all the cells are about equally good. The author thinks, therefore, that such cells may advantageously be used in measurements as standard cells, since their E.M.F. is very slightly influenced by changes of temperature. The object of introducing a large column of plaster of Paris moistened only with pure water is to increase the internal resistance of the cell, as well as to prevent the diffusion of the solution of sulphate of copper, which might otherwise attack the zinc pole.

The author has constructed similar cells with chloride of silver and zinc, but did not obtain very satisfactory results. These were better with cells in which the plaster of Paris was saturated with a supersaturated solution of nitrate of silver. Such a cell has an E.M.F. of 1·52 volt.

The decrease of the E.M.F. of these dry cells for a long-continued short circuiting only reaches 1 or 2 per cent., and on open circuit they regain their former value.

The effect of temperature on the internal resistance of these cells is shown by measurements made on cell No. 29, belonging to Group III. At 1·5° C., R = 232,300 Siemens units; at 20° C., R = 132,900 Siemens units, which gives a temperature coefficient of 0·04 per degree centigrade.

The paper concludes with details of measurements made to verify the constancy of the E.M.F. on closed circuit.

R. W. WILLSON—A SENSITIVE GALVANOMETER WITH MEASURABLE REDUCTION FACTOR.

(*Annalen der Physik und Chemie, Vol. 26, Pt. 1, p. 44, No. 9, 1885.*)

After a mathematical investigation of the action of the tangent galvanometer, which is fundamentally the same as that given by Maxwell in para-

graph 700 of his treatise, the author proceeds to a description of the particular instrument which he constructed. It consists of a coil with a magnet needle at its centre, provided with a mirror for reading by means of a fixed scale and telescope, and so far is of the usual form; it is, however, so arranged that the coil can be shifted laterally in either direction parallel to its axis, the amount of movement being measured on a scale.

The position of the coil, in which the needle is directly in its centre, can be easily determined, within a small fraction of a millimètre, by taking the mean of the several positions on opposite sides, for which the current produces equal deflections. The dimensions to be given to the galvanometer depend upon the degree of exactness with which the deflections can be determined. If we admit that with a scale and telescope we can arrive at a minimum error of $\frac{1}{10000}$ of the deflection, then, in order that the value of the distance (A) of the magnet from the plane of the coil may be determined with equal exactitude, the distance between the two extreme positions of the coil should be 100 mm., *i.e.*, $A=50$ mm. The radius of the coil is determined by the equation $A=R\sqrt{\frac{1}{4}}$, which is deduced mathematically in the first part of the paper.

The dimensions of the winding of the coil itself depend upon the nearness with which the coefficients of correction (α and β) in the equation

$$\tan.\phi = \frac{i\,n\,F}{4}\left(1+\alpha+\beta+\gamma+\&c.\right)$$

for the value of the current, can be obtained. If the windings have a rectangular section (h = height, b = breadth), and if $A=R\sqrt{\frac{1}{2}}$, then $\alpha = \frac{(b^2-h^2)}{18\,R^2}$. If, however, the height and breadth are not nearly equal, let $b-h=\epsilon$, then $\alpha = \frac{(b+h)\,\epsilon}{18\,R^2}$. If the height and breadth of the windings can be measured within 0·1 mm., then the differential of ϵ is less than 0·2 mm.; and if $R=100\sqrt{\frac{1}{2}}$, then $d\,\alpha$ becomes 0·0001 for $h=22$ mm. The coefficient β in the case of the above dimensions would be only 0·000002. Five thousand turns of wire can be wound on a ring of the dimensions given, which with one microampère of current and a distance from mirror to scale of 2½ mètres would give a deflection of 10 mm.

The magnet is fastened to the back of the mirror, which is suspended by a cocoon fibre from a torsion head, and is closely surrounded by its case, so that it acts as its own air-damper. The case is fixed on a bar, which serves at the same time as support for the disc on which the wire is wound. A vernier attached to the disc measures its displacement on a millimètre scale on the bar The whole instrument can be rotated about a vertical axis, and is provided with levelling screws. The coil consists of two double covered wires of about 0·35 mm. diameter, which are wound on close to each other, and are turned over at each fresh layer.

A. NACCARI and G. GUGLIELMO—HEATING OF ELECTRODES IN VERY RAREFIED AIR.

(*Beiblätter, Vol.* 9, *No.* 11, 1885, *p.* 751.)

Three cylindrical thin platinum electrodes were fixed in a T-shaped tube. They were 11 cm. long and 1·2 cm. thick, and were connected externally with capillary tubes in which the rise of the alcohol in the electrodes could be noted. The electrodes were each 4 cm. distant from the junction of the T tube. The middle electrode was always connected to one pole of the induction coil, and one or other of the two end electrodes to the other pole. The air was thoroughly dried by means of sulphuric acid.

For pressures from 1·76 to 0·023 mm. the heating of the negative electrode increases for decrease of pressure; the heating of the positive electrode, which is withdrawn from the radiation from the negative electrode, is very small. The heating of the insulated electrode by the radiation is not small.

For very low pressures, however, the positive electrode heated considerably, whilst the negative remained quite cold; this occurred when the electrodes were only 1 mm. apart, but not when they were 8 mm. apart (*i.e.*, each 4 mm. from the junction of the T tube). It also appears that at very low pressures the heating of the negative electrode decreases; that of the positive increases. In accordance with this is the observation of Hittorf, that for very small distances between the electrodes, and for extremely low pressures, the positive electrode is disintegrated and remains dark, in contradiction to what happens at higher pressures when the electrodes are further apart.

In some further experiments at a pressure of 0·27 mm., with a rapidly increasing current, the heating of the negative electrode increased somewhat quicker than in the former ones, whilst the heating of the positive electrode remained almost constant. Similar results were obtained when a Holtz machine was used in place of the induction coil, only the amount of heating was somewhat less.

The amounts of heat produced are for unit value of the heating of the electrodes (measured in divisions of the column of alcohol) about 0·1277 small calories (gramme-degree centigrade).

The paper contains tables giving the pressures, currents, and rise of temperature of the three electrodes.

G. POLONI—RELATION BETWEEN THE ELASTICITY AND THE CONDUCTIVITY FOR HEAT AND ELECTRICITY OF SOME METALLIC WIRES.

(*Beiblätter, Vol.* 9, *No.* 11, 1885, *p.* 743.)

A simple relation between the coefficient of elasticity and the conductivity, and their changes for regular changes of temperature of the whole wire, does not seem to exist.

If a wire is heated at one single point to a temperature (t) above that of the surrounding air; if R_0 is the conductivity before heating, and R after heating,

then $R = R_0 + \beta t$. If α is the temperature coefficient of the resistance of the wire for a steady increase of temperature, d its diameter, K_c and h its internal and external conductivity for heat, then $\beta = \frac{\alpha \sqrt{d} \sqrt{K_c}}{2 \sqrt{h}}$.

All the wires were silver-plated. If for iron at 20° C., $K_c = 0{\cdot}1653$ (Lorenz), h for silver is 0·0007923. If we put $p = \frac{\beta}{\sqrt{d}}$, D = specific gravity, K_e = the electrical conductivity, ϵ E the decrease of the modulus of elasticity (E) for each degree of increase of temperature, then

$$\frac{p\,\alpha}{\epsilon\,E\sqrt{K_c}} = \text{constant} = M; \quad \frac{p}{\epsilon\,E\sqrt{K_c}\,\sqrt[4]{D}} = \text{constant} = N;$$

hence $\alpha \sqrt[4]{D} = \frac{M}{N}$ = constant.

In reality, the value $10^6 \times \frac{M}{N}$, according to Poloni, varies for the different metals between 5,780 and 7,574; according to Arndtsen, between 4,415 and 7,046; according to Matthiessen, between 6,520 and 8,269.

If we establish the same relations for another wire, then, since $\frac{K_c}{K_e}$ at equal temperatures is constant for all wires,

$$\frac{(\epsilon^1 E^1)^2}{(\epsilon E)^2} \times \frac{\alpha}{\alpha^1} = \frac{h}{h^1}$$

where the dashed symbols refer to the second wire, and from which equation the emission constant can be determined.

The simple quotients $\frac{p}{E\,K_e}$ vary for silver, copper, platinum, and iron—both hard and soft—between 1,035 and 1,554; they therefore are not constant.

If they were constant, since $\frac{1}{K_e}$ = R is equal to the specific resistance, and $E = \frac{C\,P\,L}{l}$ for a wire of unit section is equal to the modulus of elasticity, then $p\,R = \frac{l^1\,P\,L}{l}$; that is, the increments of resistance of a wire heated at one point through 1° are inversely proportional to the elongations produced by a certain load in various wires of equal initial length.

Dr. O. FRÖLICH—THEORY OF THE DYNAMO MACHINE. PART II. COMPOUND WINDING FOR CONSTANT E.M.F.

(*Elektrotechnische Zeitschrift, Vol.* 6, *Pt.* 4, *p.* 139, *April*, 1885.)

Since the appearance of the first part of this paper I have arrived at a rule according to which any dynamo machine should be wound so as to give the greatest possible constancy of E.M.F. It is at once clear that the most important part of the compound winding for constant potential is the shunt winding, not the series. The curve of potential for a series dynamo rises

rapidly to a maximum, and then falls more slowly; the curve for a shunt machine rises always, and approaches more and more to an asymptote.

We draw for any dynamo the curves of the E.M.F., starting with that for simple-shunt winding; then a series of curves which correspond to an increasing number of series windings, viz., m_s* = 0·01, 0·02, 0·03, 0·04, 0·05, 0·06. We assume that the space occupied by the series winding is always the same; there is then a relation between the number of turns (m_s) and the resistance (S) of the series coils, which is $S = 78·1 \times m_s^2$; we imagine, therefore, this space first filled with very thick wire, and then with always thinner and thinner wire.

From an inspection of these curves (given in the original) we see that at their commencement they rise with an increasing number of series coils up to a certain point (m_s = 0·03), and afterwards descend; in the later paths of the curves there is always a rise with increased number of series coils, but of small amount. Further, it is evident that the simple-shunt curve reaches its maximum at infinity, whilst the other curves reach their maxima near the commencement; this maximum for an increasing number of turns always, however, moves further from the axis of Y, *i.e.*, towards higher external resistances.

If we attempt to calculate the maxima of the curves, we find that, for the least value of the number of series coils, the maximum remains at an infinite distance, as for the curve of simple-shunt winding, but that from a certain curve onwards the maximum occurs at the commencement.

The potential at the terminals for both kinds of compound winding (shunt parallel to armature, and shunt parallel to outer circuit) can be expressed by the formula

$$P = A \frac{R}{R + \alpha} - B \frac{R}{R + \beta},$$

where β increases with an increasing number of series coils, and becomes nil for simple-shunt winding.

If $\beta = o$, *i.e.*, in the case of a simple-shunt winding, the maximum value of P is reached only when the outer resistance (R) becomes infinitely great.

Even, however, if the series coils have a value, the maximum is still at infinity, so long as $\beta < \alpha \frac{A}{B}$.

If $\beta = \alpha \frac{A}{B}$, then we have the limiting curve, which still has its maximum at infinity; but if $\beta > \alpha \frac{A}{B}$, then the maximum occurs at the commencement of the curve.

The limiting curve is that one which most nearly approximates to a straight line. It will be preferable to choose the number of series windings somewhat greater than for the limiting curve, in order to exaggerate the first part of the curve; this increase must, however, be very small.

If P is a maximum with respect to R, then, if R^1 is the value of R corresponding to this maximum,

$$\frac{d\,P}{d\,R} = o = A \frac{\alpha}{(R^1 + \alpha)^2} - B \frac{\beta}{(R^1 + \beta)^2}.$$

* The symbols have the same meaning as in the first part of the paper.

This equation can only be solved for $R^1 = \infty$, if $\beta = o$, and so long as $\beta < a \frac{A}{B}$.

If $\beta = a \frac{A}{B}$, we have

$$o = \frac{A\, a}{(R^1 + a)^2} - \frac{A\, a}{(R^1 + \beta)^2};$$

or, dividing by $A\, a$,

$$o = \frac{2\, R^1 (\beta - a) + \beta^2 - a^2}{(R^1 + a)^2\ (R^1 + \beta)^2};$$

and, finally,

$$o = (\beta - a) \frac{2\, R^1 + a + \beta}{(R^1 + a)^2 (R^1 + \beta)^2}.$$

This equation also is only solved for $R^1 = \infty$.

If, however, β is somewhat larger than $a \frac{A}{B}$; for instance, $\beta = a \frac{A}{B} + \epsilon$, we obtain

$$o = \frac{(\beta - a)(2\, R^1 + a + \beta) - \frac{\epsilon}{\beta} (R^1 + \beta)^2}{(R^1 + a)^2\ (R^1 + \beta)^2}.$$

This equation can be solved for finite values of R^1, as well as for $R^1 = \infty$; the former correspond to the maximum at the commencement of the curve, the latter to its asymptotic path.

The limiting curve is therefore determined by the condition

$$\beta = a \frac{A}{B} \quad \dots \quad \dots \quad \dots \quad \dots \quad (1)$$

and if we choose the number of series coils somewhat larger than the value given by this condition, we shall obtain that curve for the potential at the terminals which most nearly approximates to a straight line.

If the shunt coils are parallel to the armature,

$$a = a + s;\ \beta = s + \frac{m_s}{m_p} \cdot p;$$

$$A = f\, v;\ B = \frac{p}{m_p};$$

and the expression for the limiting curve becomes

$$\frac{s + \frac{m_s}{m_p} \cdot p}{s + a} = \frac{f\, v}{\frac{p}{m_p}} \quad \dots \quad \dots \quad \dots \quad (2)$$

If the shunt coils are parallel to the outer circuit,

$$a = a + s;\ \beta = \frac{m_s}{m_p + m_s} \cdot p;$$

$$A = f\, v;\ B = \frac{p}{m_p + m_s};$$

and therefore

$$\frac{m_s}{(m_p + m_s)^2 (s + a)} = \frac{f\, v}{p^2} \quad \dots \quad \dots \quad \dots \quad (3)$$

The above formulæ (2 and 3) contain all the elements which have any effect on the working of the dynamo—the speed (v), the armature constant (f),

the resistance of the armature (a), of the series winding (s), of the shunt winding (p), and the number of turns on both (m_s, m_p).

The practical application of the above formulæ to the determination of the best kind of winding is the following:—

1. First a suitable shunt winding is found, and by means of this, without any series winding, the armature value $f\ v$ and the electro-magnet value $\frac{p}{m_p}$ are determined.

From the present point of view it is only desirable that (a) at a suitable speed this winding should give the requisite potential on open circuit; (b) the value $\frac{p}{m_p}$ should only be a fraction (at most $\frac{1}{2}$) of the value $f\,v$. For the present purpose also we need not consider the energy absorbed and the heat produced in this winding. In order to determine the quantities $f\ v$ and $\frac{p}{m_p}$ with the simple-shunt winding, several trials are made on open circuit at varying speeds. We then have the simple formula

$$P_p = f\,v - \frac{p}{m_p}$$

(the quantity $\frac{a}{p}$ can be neglected as small in comparison to 1). The values of P_p are drawn as ordinates, the speeds as abscissæ, and the straight line drawn which best agrees with the observed values; from any two points in this line we can then obtain $f\ v$ and $\frac{p}{m_p}$.

2. Having found a suitable shunt winding, an experimental series winding is added, and some experiments made at the same speed as before, but with less resistance in the outer circuit, and therefore with a larger current, and the number of turns (m_s) determined from the experiment, as well as the values of the resistances a, s, and p. For instance, if the shunt windings are parallel to the armature,

$$m_s = -\frac{m_p}{p}(R + s) + \frac{a + R + s}{f\,v - I\,(a + R + s)};$$

of course the mean value of m_s found from several experiments would be taken.

3. If m_s'and s for the experimental winding are known, the relation between m_s and s is fixed, and introduced into the equations (2) and (3), and a series winding corresponding to the limiting curve is calculated; for the actual series windings to be put on the machine we then choose a somewhat higher number.

The connection between m_s and s is a matter of choice. We can, as above, consider the space to be filled by the series windings as constant, so that

$$m_s^2 = c \cdot s;$$

or we may take the sectional area of the wire as constant, so that

$$m_s = c^1 \cdot s.$$

Dr. H. HAMMERL—BEHAVIOUR OF RING MAGNETS VARIOUSLY WOUND.

(Elektrotechnische Zeitschrift, Vol. 6, No. 9, Sept., 1885, p. 378, and No. 10, Oct., 1885, p. 474.)

The strength of the temporary magnetism in the ring was measured by the current induced in a coil which surrounded it, the ring being rotated on an ordinary whirling table through 180°, through the fixed coil.

The first experiments were made with two soft iron rings 13 cm. in diameter inside, both being wound throughout with a continuous helix of wire. Though both were identical in shape, size, and weight, and had the same number of turns, and were excited by the same current, it was found that one (A) was always less powerful than the other (G) for all the current strengths employed.

The ring A having had its permanent magnetism entirely destroyed, was then wound with only two coils, diametrically opposite and consisting of a few turns of wire, each coil occupying $\frac{1}{36}$ of the entire circumference of the ring, and the two being joined up parallel. On comparison with the ring G, which had not been altered, it was found that the new winding produced, for the same magnetising force, twice the magnetism in A which the old winding produced in G.

The next point was to vary the breadth of the two coils on ring A, when it was found that the wider the coil the less the temporary magnetism, and that the more the windings are arranged in diametrically opposite coils, as in the Gramme helix, the greater is the temporary magnetism. This result is also shown graphically by figures of the lines of forces produced by both kinds of winding, as shown by iron filings.

The author then studied the behaviour of a ring with several pairs of coils, arranged round the ring at equal distances apart, and found that with the same magnetising force the ring always receives a smaller magnetic moment the greater the number of pairs of coils connected up, since several pairs of coils act in the same way as one pair of greater width.

If the pairs of coils are successively shunted into the circuit, but the current is kept constant—for example, by taking out corresponding resistances in the external circuit—the magnetising force will increase proportionately to the number of pairs. In this case the temporary magnetism of the ring will always increase, but each time by a smaller quantity, for each increase in the number of pairs of coils in action. Not only the magnetising force is altered, but with it the width of the coils. If, however, the magnetising force is kept constant, then for every pair of coils added the magnetism diminishes, since this is the same thing as adding to the width of the coil.

In an experiment in which the pairs of coils were successively introduced into the circuit from one up to six, but connected up parallel, a maximum magnetic moment was produced with three pairs of coils, after

which the magnetic moment fell off, although the magnetising power increased.

The author has made further researches by altering the positions of the pairs of coils, and by using three or nine coils, the results of which are given in tables and diagrams.

ANON.—ELECTRICALLY-WORKED CLUTCH FOR SHAFTS.

(*Electrotechnische Zeitschrift, Vol.* 6, *No.* 11, *Nov.*, 1885, *p.* 497.)

The arrangement can be adapted to any form of mechanical clutch, and is intended to be used in cases where it is desirable to release the clutch from a distance.

To the lever which throws the clutch in or out of gear is attached a rod, the other end of which is fixed to a pin on the periphery of a disc. The disc turns on a central pivot, and a strong spiral spring, attached at one end to the rod and at the other to a point on the periphery of the disc, tends to bring this point of attachment over towards the rod, and in so doing actuates the lever working the clutch.

The disc is rotated by hand through a certain angle, whereby the spiral spring is stretched; and it is held in this position by an arrangement of levers and clicks inside the apparatus, which also contains an electro-magnet. On passing a current through this electro-magnet the armature is attracted, releasing the clicks and levers, and allowing the spiral spring to contract and to work the main lever of the clutch.

G. S. JENMAN—BALATA AND THE BALATA INDUSTRY IN BRITISH GUIANA.

(*Report to Government.*)

That part of Guiana between the Berbice and Corentyne rivers is drained by the Canje, and is a low alluvial forest region, abounding in the bullet-tree as well as other rubber and gutta trees. On the banks of this Canje river the industry of balata collecting has grown up during the last 25 years.

Balata is obtained from the bark of the bullet-tree (*Mimusops balata*), of the order *Sapotaceæ*, to which also belong the *Dichopsis gutta* and several other gutta-percha trees of the Malayan Archipelago, which are the principal sources of the present market supply of gutta. The bullet-tree does not inhabit the higher ground, but flourishes in the lowlands, reaching at maturity a height of 120 ft., with large head and a cylindrical trunk 60 to 70 ft. long and 4 to 5 ft. in diameter. The wood has a reddish tinge, and is one of the hardest and densest in the colony, weighing 80 lbs. to the cubic foot. It is very durable, useful, and obtainable in scantlings 3 ft. 6 in. square; but the largest now cut square about 30 in., being 50 to 60 ft. long, and used for the foundations of buildings and machinery on the sugar estates. The bark consists of three primary layers: the outer layer is dark brown, hard and dry; the next, usually

much thicker, is spongy in tissue and lactiferous, of a reddish raw-beef colour, and yields the balata milk; whilst the inner one is thin, more ligneous, of a brown wood colour, and with fewer lactiferous vessels. To get the milk the bullet-tree is tapped by cutting narrow V-shaped channels in the bark; with care, this may be done on opposite sides of a tree every alternate year without inflicting any permanent injury. A tree 15 to 20 in. in diameter bled 8 ft. high will yield three pints, and twice as much when also bled higher up. When felled, a trunk 40 ft. long, that would square 12 to 15 in., would give about two gallons of milk; whilst a young tree 1 ft. diameter will yield a gallon; the largest and best yielding trees give four or five gallons. Hollow trees yield better than sound ones. If not collected at time of felling, the milk will ascend to the branches, leaving the bark quite dry after one day, unless a ring be cut round the trunk. When collected, the milk may be kept for days, and is dried in shallow wooden trays by the agents at the settlements along the river. As the upper layers form a crust ¼ in. it is hung up to drip and dry. One gallon balata milk weighs about 10 lbs., and on drying yields 4 to 6 lbs. of pure balata. In a fairly good sample of balata milk, 100 parts by weight contained—water, 39·04; balata, 60·31; mineral matter, 0·65. The milk is dazzlingly white, and in drying by evaporation, exposed to the air, it turns dark brown externally, but when precipitated by spirits of wine it remains white, although it is not so good. As at present carried out, drying by evaporation is a slow and tedious operation. Some quicker system is very desirable. The best, on a small scale, is to dry in *leaden* trays by means of steam at 100° C. Milk of specific gravity 0·9868 yielded 60·18 per cent. solid matter forming balata gum, and 0·48 per cent. mineral. The ordinary gum, prepared by evaporation, contains about 13 per cent. foreign solid matter. Three samples of bark—1st, from tree where tapped, contained 6·93 per cent. gum; the 2nd, from tree *not* tapped, gave 9·19 per cent.; and 3rd, from a tree 14 days after being felled, contained 10·80 per cent. gum. The collectors get four shillings per gallon for pure milk, and the market price of clean, well-dried balata is about 1s. per lb. With fair weather a man can earn from one to five dollars a day collecting milk.

Balata is intermediate in character between india-rubber and gutta-percha. It combines the properties of both these materials, and is as good as the best combination of them. It is very strong and does not stretch much under tension, and hence makes excellent bands for machinery. According to Dr. Hugo Muller, F.R.S., although probably identical in chemical composition, balata and gutta-percha are distinctly different in physical character. Balata is softer at ordinary temperatures, and not so rigid in the cold. When exposed to the influence of the atmosphere, light and air, gutta-percha becomes altered on the surface, and the whole mass is gradually converted into a brittle resinous substance; whereas balata is but slowly acted upon, and remains supple and coherent.

The electrical insulating quality of balata is quite equal to that of gutta-percha. It is clear that intrinsically balata is one of the most valuable substances of the kind known, and its want of success is due to the hitherto

limited and uncertain supply, so as not to enable any firm to use it on a large scale and invest in the special machinery required for its manufacture.

The milk of other plants is sometimes collected and mixed with the balata milk. Of these the *Touck-pong* tree yields true india-rubber, worth 2s. 6d. per lb.; the *Barta-balli* tree milk contains more water, and the caoutchouc gum obtained from it by evaporation is soft and sticky, and inferior to balata; a third caoutchouc plant is a bush rope or climber, and is said to yield a plentiful supply of milk and very good gum.

The author points out the great damage done to bullet-trees by careless tapping, as well as the wanton destruction of trees for illicit trade, the balata obtained bearing only a very small proportion to the value of the timber destroyed. With a view to the better conservation of this valuable timber, he lays suggestions before Government for the new forest law which is in contemplation.

LIST OF UNABSTRACTED ARTICLES.

(*Comptes Rendus, Vol.* 100.)

No. 21.—**G. PLANTÉ**—Particular Properties of the Electric Current produced by the Rheostatic Machine. **A. CROVA and P. GARBE**—Determination and Registration of the Charge of Accumulators.

No. 22.—**H. BECQUEREL**—Measurement of the Magnetic Rotary Power of various Bodies in Absolute Units.

No. 23.—**E. HOSPITALIER**—Measurement of Commutated Currents.

No. 25.—**BLAVIER**—Influence of Storms on Underground Telegraph Lines.

No. 26.—**FAYE**—Palmieri's Researches on Atmospheric Electricity. **MASCART**—Note on the preceding. **CRULS**—Secular Variation of the Magnetic Declination at Rio de Janeiro. **LEVAT**—A New Tin-Copper Battery.

(*Vol.* 101.)

No. 1.—**FAYE**—Answer to Mascart's Note (see *ante*).

No. 3.—**CROVA and GARBE**—Charge and Discharge of Accumulators. **G. FOUSSEREAU**—Resistance of Alcohol.

No. 5.—**BOUTY and FOUSSEREAU**—Use of an Alternating Current in the Measurement of Liquid Resistance.

No. 6.—**A. MILLOT**—Oxidation Products of Carbon by the Electrolysis of a Solution of Ammonia.

No. 8.—**DECHARME**—Analogies between Electro-chemical and Electrodynamical Rings.

No. 9.—**ZENGER**—Magnetic Storms and Auroræ Boreales.

(*L'Electricien, Vol.* 9.)

No. 109.—**E. PIAZOLLI**—Installation at Milan.

No. 110.—**L. DESRUELLES**—Batteries with Bichromate and Nitric Acid.

No. 111.—**P. JABLOCHKOFF**—New Battery. **E. HOSPITALIER**—Portable Lamp of Larochelle.

No. 112.—**E. HOSPITALIER**—Industrial Measuring Apparatus. **R. SEGUELA**—Registering Apparatus for Speed of Trains. **F. DE MARE**—The Cruto Glow Lamp.

No. 115.—**E. HOSPITALIER**—Calorimetric Voltmeters. **F. DE MARE**—The Cruto Glow Lamp.

No. 116.—**E. HOSPITALIER**—The Causes which necessitate the frequent Re-calibration of Instruments. **F. DE MARE**—Circulating Bichromate Battery.

No. 117.—**R. ARNOUX**—Proposal for a Unit of Current Density. **R. SEGUELA**—Registering Apparatus for Speed of Trains.

No. 119.—**E. HOSPITALIER**—Batteries with High E.M.F.

No. 120.—**E. HOSPITALIER**—Self-regulating Machines.

No. 121.—*Anon.*—Practical Calibration of Ammeters. **E. HOSPITALIER**—Electrolytic Refining of Black Copper.

No. 122.—**E. HOSPITALIER**—New Inductive Transformers with a Closed Magnetic Circuit.

No. 123.—**E. HOSPITALIER**—Distribution of Electricity by the Zipernowski-Déri System.

No. 124.—**E. HOSPITALIER**—Magneto Machines for Lighthouses. *Anon.*—Electrolytic Extraction of Gold.

No. 127.—**L. LOSSIER**—Transmission of Power.

No. 128.—**E. HOSPITALIER**—Thermo-dynamic Efficiency of Thermopiles.

(*Journal de Physique, Vol.* 4, 1885.)

April.—**HURION**—Heat Effects of Electric Sparks.

May.—**G. FOUSSEREAU**—Electrical Resistance of Insulators. **A. POTIER**—The Contact Theory. **MESLIN**—On the Dimensions of E.M.F. and of Potential.

June.—**H. PELLAT**—E.M.F. of Combustion, and Method of Ascertaining the Potential of the Air.

August.—**MASCART**—Theory of Gramme Dynamo.

September.—**BOUTY and FOUSSEREAU**—Use of Alternating Currents in the Measurement of Liquid Resistance.

(*La Lumière Electrique, Vol.* 16.)

No. 22.—**G. FERRARIS**—Theory of Gaulard & Gibbs' Secondary Generators. **M. COSSMANN**—Railway Signals. **P. NÉZERAUX**—Accumulators for Electric Railways.

No. 23.—**W. H. WEBER**—Domestic Telephones. **M. COSSMANN**—(See *ante.*) **G. FERRARIS**—(See *ante.*) **B. MARINOVITCH**—Telephone of Mildé.

No. 24.—**C. DECHARME**—Application of Electricity to the Study of Vibrating Bodies. **G. RICHARD**—Telpherage.

No. 25.—**G. SZARVADY**—Conditions to be fulfilled by Glow Lamps. **G. FERRARIS**—(See *ante.*) **Dr. R. WEBER**—Microphone of Dr. Hipp. **A. MINET**—Classification of Instruments for the Measurement of Current. **A. ROUILLIARD**—Alarm Bells for Safes.

No. 26.—**C. DECHARME**—(See *ante.*) **G. FERRARIS**—(See *ante.*) **E. RADKIEWITCH**—Watchman's Control.

(*Vol.* 17.)

No. 27.—**G. SZARVADY**—Life of Glow Lamps. **G. MARESHALL**—Water Level Indicator.

No. 28.—**M. COSSMAN**—(See *ante.*) **G. SZARVADY**—(See *ante.*) **J. RAVAGLIA**—Hydrometrograph.

No. 29.—**M. COSSMAN**—(See *ante.*)

No. 30.—**G. FERRARIS**—Experiments with the Zipernowski-Déri Transformers. **M. COSSMANN**—(See *ante.*) **G. SZARVADY**—Characteristic Curves for Glow Lamps.

No. 31.—**A. ROUILLIARD**—Measurements of Submarine Cables. **M. COSSMANN**—(See *ante.*)

No. 32.—**R. CLAUSIUS**—Dimensions of Molecules and their Distance Apart. **M. COSSMANN**—(See *ante.*) **A. MINET**—Study of Galvanometers. **G. RICHARD**—Details of the Construction of Glow Lamps.

No. 33.—**C. DECHARME**—Analogies between Electrical and Hydrodynamical Phenomena. **M. COSSMANN**—(See *ante.*)

No. 34.—**J. BERTRAND**—Military Telegraphs in the European Armies. **C. DECHARME**—(See *ante.*) **M. COSSMANN**—(See *ante.*) **F. UPPENBORN**—Some New Mirror Galvanometers.

No. 35.—**P. MARCILLAC**—The Observatory of Vesuvius. **J. BERTRAND**—(See *ante.*) **A. MINET**—(See *ante.*)

No. 36.—**Dr. TOBLER**—Mance's Method for Lightning Rods. **C. DECHARME**—(See *ante.*) **G. RICHARD**—Some Recent Telephones.

No. 37.—**G. RICHARD**—Details of the Construction of Glow Lamps. **A. MINET**—(See *ante.*)

No. 38.—**W. C. RECHNIEWSKI**—Telegraphy by Induction. **C. DECHARME**—(See *ante.*) **G. RICHARD**—New Determination by an Electrical Method of the Mechanical Equivalent of Heat.

(*Annalen der Physik und Chemie, Vol.* 25.)

No. 2.—**E. PFEIFFER**—Conductivity of Mixtures of Water and Alcohol. **C. L. WEBER**—Conductivity and Temperature Coefficient of Solid Mercury. **K. STRECKER**—Reproduction of the Siemens Unit. **J. W. GILTAY**—A New Electro-Dynamometer.

No. 3.—**K. STRECKER**—(See *ante.*) **J. ELSTER and H. GEITEL**—Very Simple Influence Machine. **E. RIECKE**—Electro magnetic Rotation of a Liquid. **J. FREYBERG**—Töpler's Magnetic Balance. **H. JAHN**—Work Done in Electrolysis by a Current.

No. 4.—**E. BUDDE**—The Quantity of Electrical Elementary Molecules. *Idem*—Deduction from Gauss's Electro-dynamic Law. **W. KÖNIG**—Influence of Magnetisation on Fluid Friction. **E. LOMMEL**—New Influence Machines. **TH. HÄBLER**—An Error in the Theory of the Gold-leaf Electroscope.

(*Beiblätter, Vol.* 9.)

No. 6.—**H. HESEHUS**—An Ammeter dependent on the Peltier Effect. **A. PACCINOTTI**—Artificial Magnetisation of Magnetite. **D. GOLDHAMMER**—Discharge in Rarefied Gases. **P. CARDANI**—Influence of the Capacity of a Condenser on the Cross Section and Duration of the Spark.

No. 7.—**G. GUGLIELMO**—Influence of the Extra Current on the Measurement of the Resistance of Batteries by Mance's Method. **H. GÖTZ and A. KURZ**—Resistance of Wires under Tension. **W. OSTWALD**—Conductivity of Acids. **KALISCHER**—New Secondary Battery. **TH. GROSS**—New Production of Currents by Magnetism. **G. GUGLIELMO**—Heating of Electrodes of Induction Coils in very Rarefied Air. **W. C. RÖNTGEN**—Researches on the Electro-magnetic Action of Dielectric Polarisation.

No. 8.—**ASCOLI**—Method of Calibrating a Bridge Wire. **G. CANTONI**—Polarisation of Dielectric Plates. **NEESEN**—Experiments with Geissler's Tubes.

No. 10.—**W. STSCHEGLAJEFF**—The Electrolytic Figures of Nobili and Guebhard in a Magnetic Field. **G. VINCENTINI**—Conductivity of very Dilute Solutions.

(*Centralblatt für Elektrotechnik, Vol. 7.*)

No. 14.—**Dr. KRÜSS**—Petroleum Lamps as a Secondary Standard for Photometry.

No. 15.—**T. MARCHER**—Theory of Glow Lamps.

No. 16.—**F. UPPENBORN**—Unipolar Machines. **E. KITTLER**—Measurements of Current Strength.

No. 20.—**F. UPPENBORN**—(See *ante.*) **v. TAUND-SZYLL**—New Differential Selenium Recorder.

No. 21.—**CHAPERON**—Polarisation of Oxidisable Metals.

No. 22.—**W. PEUKERT**—Conversion of Electrical Energy into Light and Heat in Arc and Glow Lamps.

No. 24.—**A. WACKER**—Parallel Connection of Arc and Glow Lamps on the same Circuit.

(*Elektrotechnische Zeitschrift, Vol. 6.*)

No. 7.—**Dr. B. WEINSTEIN**—Earth Currents. **WEBER**—Statistics of Lightning Strokes in Saxony. **Dr. A. TOBLER**—Sir Wm. Thomson's Siphon-Recorder. **RÜHLMANN**—Secondary Generators or Transformers. *Idem*—Electric Lamp for Projection Apparatus. **L. SCHARNWEBER**—Electric Light in Photography.

No. 8.—**O. VON MILLER**—The German Edison Central Station. *Anon.*—Electric Storms on Telegraph Lines. **Dr. B. WEINSTEIN**—(See *ante.*) **Dr. A. TOBLER**—(See *ante.*) **ZETSCHE**—Three Telephone Exchanges on One Line Wire. **Dr. ULBRICHT**—Carbon Lightning Dischargers.

No. 9.—**Dr. B. WEINSTEIN**—(See *ante.*) **J. FREYBERG**—Danger from Lightning in Saxony. **Dr. A. TOBLER**—(See *ante.*) **RÜHLMANN**—Secondary Generators. **Dr. HORN**—Electrical Speed Counter.

No. 10.—**P. NIPKOW**—The Telephotograph and the Electrical Telescope. **G. FERRARIS**—Transformers of Zipernowski-Déri System. **W. SIEMENS**—Improvements in the Efficiency of Glow Lamps. **GERLAND**—Improvements in Dynamos.

(*Zeitschrift für Elektrotechnik, Vol. 3.*)

No. 11.—**L. KOHLFÜRST**—Electric Railways at the Vienna Exhibition **O. VOLKMER**—Electro-deposition with Schuckert Machines. **R. v. FISCHER TREUENFELD**—Military Telegraphs. **Dr. R. LEWANDOWSKI**—Use of Induction Apparatus in Medicine. **C. ERNST**—Electrolytic Extraction of Gold.

No. 12.—**L. KOHLFÜRST**—(See *ante.*) **G. A. SCHILLING**—The Screening Effect of Iron. **R. v. FISCHER TREUENFELD**—(See *ante.*) **O. VOLKMER**—(See *ante.*) **LEWANDOWSKI**—(See *ante.*) **V. v. LANG**—Measurement of E.M.F. of the Electric Arc.

No. 13.—**L. KOHLFÜRST**—(See *ante.*) **G. A. SCHILLING**—(See *ante.*) **LEWANDOWSKI**—(See *ante.*)

No. 14.—**L. KOHLFÜRST**—(See *ante.*) **LEWANDOWSKI**—(See *ante.*)

No. 15.—**L. KOHLFÜRST**—(See *ante.*) **LEWANDOWSKI**—(See *ante.*) **PULUJ**—Cohesion and Density of Glow Lamp Filaments.

No. 16.—**L. KOHLFÜRST**—(See *ante.*) **PULUJ**—(See *ante.*) **LEWANDOWSKI**—(See *ante.*)

No. 17.—**H. HAMMERL**—Relative Values of Magnetic Moments of Variously Wound Ring Magnets. **W. PEUKERT**—Calorimetric Measurements of Glow Lamps. **F. EXNER**—New Method of Determining the Size of Molecules.

INDEX TO VOL. XIV.

1885.

ABSTRACTS.